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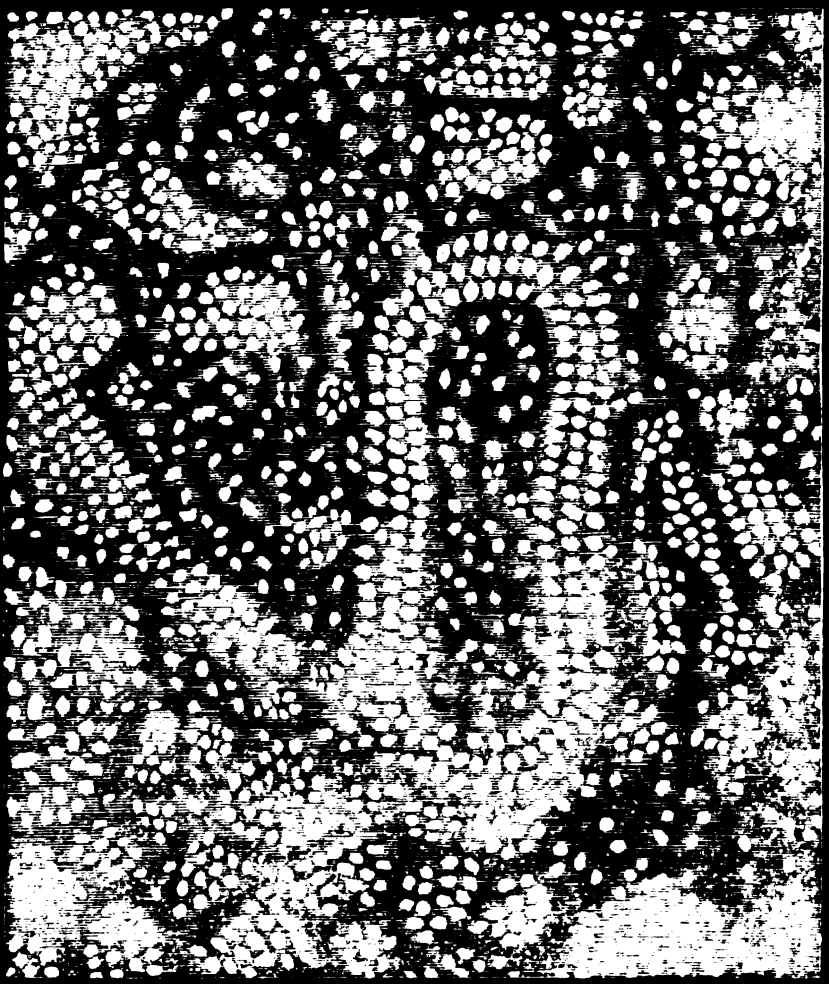

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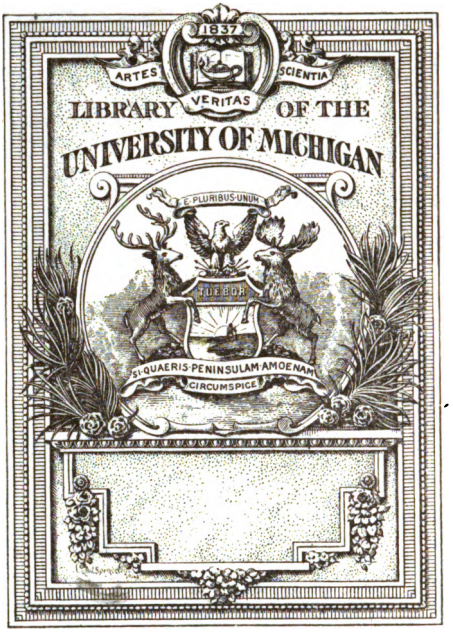

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

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FROM NOVEMBER 1865, *TO JUNE* 1866.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *November* 10, 1865. No. 1.

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WARREN DE LA RUE, Esq., President, in the Chair.

Alfred Dawson, Esq., The Cedars, Chiswick;
Charles Stewart, Esq., Akendon House, Alton, Hants, and
L. Barnett Phillips, Esq., 35 Hunter Street, Brunswick Square,

were balloted for and duly elected Fellows of the Society.

On the Comets of 1677 *and* 1683; 1860 *III.*, 1863 *I., and* 1863 *VI.* By M. Hoek.

§ 1. In a former paper I attempted to prove that, before taking their orbits under the influence of the Sun's attractions, the Comets 1860 III, 1863 I., and 1863 VI., formed a system, that is to say, at short distances from each other they had initial movements of the same direction and velocity.

That direction is nearly indicated by the straight line uniting the Sun to γ *Hydri*.

At the end of the same paper I promised to extend my researches on the Comets that appeared before 1844.

The following table contains all the cases since the year 1556, in which the question may arise about a cometary system, in consequence of the successive apparition of comets whose aphelia approach each other on the sphere. In these

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investigations I have adopted, as a limit of time the interval of ten years, as a limit of distance the angle of 10°. I confess that there is something arbitrary in these limits, but I have preferred in the beginning not to extend them too much. Moreover, I warn my readers that they may perhaps meet in this table with some combinations in which the distance somewhat surpasses the 10°, because I have only measured these distances on the globe, in order to save the time which would be required for the calculations.

I find:—

Comets.	Direction of Motion.	Aphelion. Long.	Aphelion. Lat.	Notes.
		°	°	
1672	Dir.	279·4	− 69·4	A case analogous to that of the comets of 1845 and 1846. Two retrograde motions, with one direct one. Probably the comet of 1677 is a stranger to a system formed by the two others.
1677	Ret.	286·4	− 75·7	
1683	Ret.	290·8	− 83·0	
1689	Ret.	90·1	+ 0·6	The orbit of the Comet of 1689 is rather uncertain. I have adopted the elements of Vogel.
1698	Ret.	90·8	+ 0·6	
1785 II.	Ret.	67·8	− 52·9	
1790 III.	Ret.	72·5	− 50·7	
1813 II.	Ret.	38·6	+ 24·7	
1822 III.	Ret.	46·2	+ 31·3	
1818 I.	Dir.	273·8	+ 8·4	The orbit of the Comet 1818 I. is rather uncertain.
1818 III.	Ret.	275·4	+ 10·5	
1830 I.	Dir.	31·8	− 2·1	
1835 I.	Ret.	28·0	+ 4·6	
1842 II.	Ret.	181·2	+ 56·6	
1851 IV.	Dir.	193·1	+ 61·2	
1844 II.	Ret.	9·8	+ 22·9	
1845 II.	Dir.	1·9	+ 21·0	
1845 I.	Dir.	280·5	− 41·6	In the § 3 of my former paper I have already indicated that these comets answer only by couples to the fixed limit, and that also they do not answer to the second conditions of having in their orbits a single point of intersection. Probably 1846 V. is a stranger to a system that may have contained the two other bodies.
1846 V.	Ret.	275·3	− 55·4	
1846 VIII.	Dir.	281·0	− 49·5	
1846 VII.	Ret.	340·7	− 28·9	
1847 II.	Ret.	347·4	− 31·7	

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Comets.	Direction of Motion.	Aphelion. Long.	Lat.	Notes.
1854 II.	Ret.	347·7	− 76·2	
1858 IV.	Ret.	12·9	− 76·7	
1854 V.	Dir.	345·7	+ 13·5	
1861 III.	Ret.	347·3	+ 18·2	
1855 I.	Ret.	35·0	+ 28·1	
1861 I.	Dir.	36·6	+ 32·9	
1857 III.	Ret.	57·7	− 38·0	
1857 V.	Ret.	53·7	− 42·9	
1857 VI.	Ret.	222·9	− 37·7	
1860 II.	Dir.	219·2	− 29·4	
1860 III.	Dir.	303·1	− 73·2	System the discussion of which is given in the former paper.
1863 I.	Dir.	313·2	− 73·9	
1863 VI.	Dir.	313·9	− 76·4	
1862 II.	Ret.	119·6	− 3·6	Combination overlooked in the former paper.
1864 II.	Ret.	124·2	− 0·9	

The harvest is nothing less than rich. To the ten cases belonging to the years 1844–65, the 288 preceding years have only added seven new ones, of which, moreover, two depend on orbits that are less well known. We might have foreseen it. The period 1556–1764 contains in my calculations only 46 comets; that of 1764–1840 only 72 comets; whilst the same number, 72, have appeared in the years 1840–65. Therefore the number of well-observed comets is 0·22 annually in the first period; 0·95 in the second; and 2·9 in the third.

It was an exception when, before 1700, a comet was discovered whose aphelion distance from the Sun surpassed somewhat unity, whilst in the period 1840–65 the number of aphelion distances larger than unity is a third of the whole.

On the one hand, thus astronomers have, by means of their powerful instruments, extended the sphere in which these bodies are detected and observed; on the other hand, the heavens have been explored in the last twenty-five years with a vigilance formerly unknown.

The first-fruit of it has been the discovery of several periodical comets; a second one is the knowledge of the cometary systems.

§ 2. Let us return to our table of concordant aphelia.

How can we distinguish between the cases in which there

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are systems and those where there is a fortuitous coincidence? One case only in this table allows of a direct investigation. It is that of the Comets of 1672, 1677, and 1683. First, let us examine if their orbits have a common point of intersection.

The calculation, with Halley's elements, gives—

Comets.	Intersectional Points. Long.	Lat.	
	°	°	
1672 and 1677	275·5	− 72·8	Mean Equinox of 1677·0.
1672 and 1683	236·9	− 82·4	
1677 and 1683	315·9	− 78·8	

and these comets formed thus no system, what we had already presumed from the divergency of their motions.

But how is it with those of 1677 and 1683, that have both a retrograde motion?

We may invoke here a new principle. In my former paper I have indicated that we had commonly to seek for the focal star by which the system was sent to us, in the vicinity of the intersectional point, common to the orbits of all the members of the system. Now, if we presume that two comets have formed a system before approaching the Sun, we must calculate the position of the point of intersection of their orbits; and, if this point coincide with any other known to us as a centre of cometary emanations, we may almost rest assured that these comets formed a system, the origin of which is to be found in the direction of the intersectional point.

It is the case we have here to do with. Let us reduce the point of intersection of the Comets 1677 and 1683 to the mean equinox of 1864·0, and compare it with those belonging to the cometary systems of 1860 and 1863. We obtain—

Comets.	Points of Intersection. Long.	Lat.	
	°	°	
1677 and 1683	318·5	− 78·8	Mean Equinox of 1864·0.
1860 III. and 1863 I.	316·7	− 76·5	
1860 III. and 1863 VI.	312·3	− 75·7	
1863 I. and 1863 VI.	320·8	− 78·7	

After this new coincidence I do not hesitate to express my opinion that in the vicinity of the point

$$\lambda = 319° \qquad \beta = -78°{\cdot}5$$

there must be some star that has sent in the direction of our Sun—first, the comets of 1677 and 1683, secondly, those of 1860 and 1863.

§ 3. In order to justify this opinion, let us make our calcu-

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lation of probabilities. When we consider the coincidence of two intersectional points within 2° as a result of chance, its probability is 0·0003. *A priori* we had, therefore, to expect that we should meet with it in a number of 3333 cases, and it occurs in 20 cases.

Moreover, a phenomenon of so small a probability is found to be united to another one that we could as little have had the mathematical expectation of meeting with in the limited number of cases actually considered. I mean the coincidence, within a small circle of a radius of 3°, of the aphelia of three comets that appeared in the course of $3\frac{1}{2}$ years. The probability of this phenomenon being only 0·00000049, we could expect to find it once in 2,050,000 cases, and all our comets furnish only 6,600 cases.

I will pass over in silence the mutual intersection of the same three orbits within a circle of a radius of 1°·5, as well as the limited distance of 2°·5 from the average aphelion to the average intersectional point.

The opinion is already sufficiently justified that the compound event we are considering depends on a physical cause. Is the explanation I have given the right one? Later investigations on the comets that are to appear will decide it. For the present I do not see how to arrive at any other conclusion, and I will proceed to subject the comets of 1677 and 1683 to another trial.

§ 4. Were their distances from the Sun formerly nearly equal?

The formula

$$t = C\,(r + 2q)\sqrt{r - q}$$

with its differential formula

$$dr = \frac{2}{3C}\sqrt{\frac{r-q}{r}}\,dt$$

which suppose a parabolical motion, and in which

$\log C = 8{\cdot}875232 - 10$ gives the time in years,
$\log C = 1{\cdot}437812$ gives it in days,

give me for the distances expressed in radii of the terrestrial orbit:—

Gregorian Date.	Distance from the Sun. Comet 1677.	Comet 1683.
573·86	600	601·97
837·78	500	502·18
1076·54	400	402·43
1286·93	300	302·89
1464·68	200	203·59
1602·00	100	105·14

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and there is no objection, therefore, from that side.*

§ 5. Several questions may arise with respect to the facts I have just proved.

Firstly, there is the point

$$\lambda = 319^\circ \qquad \beta = -78^\circ{\cdot}5$$

whose spherical co-ordinates referred to the equator are

$$\alpha = 4^h\ 3^m{\cdot}5 \qquad \delta = -72^\circ{\cdot}0$$

and which was called the point P′ in my former paper.

We might ask ourselves if there is any interest in searching for a star of well-defined parallax in that direction. As for me I expect that such a star will be found at a few degrees distance from the point P′, for generally we may admit that the stars whence comets come to our Sun are the nearest to us. It would not even be necessary to look for such a star P around the point P′, for it has already been demonstrated in § 7 of my former paper that, in order to come from P′ to P, we must follow on the sphere the average orbit of the comets of 1860 and 1863, and follow it in the direction of the direct movement.† In other terms, and more generally, if we call M the point

* The last table gives an idea of the manner in which the bodies of this system were separated under the influence of the Sun. Perhaps some of my readers like to have before them a similar table for the systems of 1860 and 1863. It is this—

	Distances from the Sun.		
Gregorian Date.	Comet 18′0 III.	Comet 1863 I.	Comet 1863 VI.
756·97	600	600·42	600·25
1020 87	500	500·56	500·36
1259·57	400	400·67	400·55
1470·01	300	300·86	300·80
1647·78	200	201·15	201·20
1785·10	100	101·83	102·11
1833·70	50	52·76	53·35
1853·60	20	24·43	25·52
1857·98	10	15·92	17·36

As to both tables I must remark that the distances, calculated in the supposition of parabolical orbits, are only approximate. In order to obtain more correct numbers it would be necessary to make investigations about the excentricity of each of the orbits. As to that point compare § 10.

† I must beg my readers to consider the following part of this paragraph as an erratum on the latter part of the § 7 of my former paper, that is to say, on all what follows in the mentioned § 7, the same words "*in the direction of the direct movement.*" As soon as I detected the error contained in that part I wrote to the Astronomical Society, but it appears that my letter only reached the Society after the printing of my paper.

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from which the planetary system removes, P′ P M is a great circle on the sphere, and P lies between P′ and M.

If the point P were discovered we should be able to deduce the velocity of the proper motion of our Sun, on which alone depends the distance P P′.

In order to elucidate this let us suppose that P P′ is found to be 5°, and remember that the proper motion of the Sun, taken in an opposite direction, marks on the sphere the point

$$\lambda = 76^\circ\,42' \quad \beta = -62^\circ\,57' \quad \ldots \text{ Mean Equinox of } 1864{\cdot}0.$$

which is distant from P′ by 33° 50′, or, in round numbers, 34°. Now, if we call V the velocity of the comet on its entrance into the sphere of attraction of the Sun, v the velocity of the planetary system, we shall have—

$$v : V = \sin 5^\circ : \sin 29^\circ,$$

or

$$v = 0{\cdot}180\ V;$$

and V itself being 0·367 yearly for an excentricity = 1·001 of orbit of Comet 1860 III., the result is

$$v = 0{\cdot}066 \text{ radii of the Earth's orbit yearly.}$$

I confess that this reasoning is only an example of calculation based on arbitrary suppositions, but it is proper to show the consequences that may be derived from the knowledge of the new facts.

§ 6. We might further put the question if the five comets that were sent to us by this star have left it simultaneously; or, if rather we ought to consider them as despatched at two different times.

It appears difficult to answer that question satisfactorily, but we are able to make researches on the possibility of the circumstances supposed by each of these hypotheses.

Let us, in order to make a trial of the first one, admit—

1. That the parallax of the star is 1″, or its distance 206265 unities.
2. That its attraction becomes imperceptible at a distance of 6265 unities, so that there remained 200,000 unities to be traversed by the comets after their having left the star.
3. That the comets of 1860 and 1863 have left it with velocities that were exactly equal, and of such an amount that the orbit of the Comet of 1860 III. obtains an excentricity of 1·001.

The equations of hyperbolic movement (*Theoria Motus Corporum Cœlestium*, § 21 and 22),

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$$u = \frac{\cos\frac{1}{2}(v - \psi)}{\cos\frac{1}{2}(v + \psi)} = \frac{2v}{p}\,\frac{\cos^2\frac{1}{2}(v - \psi)}{\cos\psi},$$

$$\tfrac{1}{2}\lambda e\left(u - \frac{1}{u}\right) - \log u = \frac{\lambda k t}{b^{\frac{3}{2}}}$$

give, for the case of a very large r, the approximate formulæ,

$$u = \frac{2v}{p}\,\frac{\sin^2\psi}{\cos\psi} = \frac{2v}{be}$$

$$\tfrac{1}{2}\lambda e u - \log u = \frac{\lambda k t}{b^{\frac{3}{2}}}$$

or actually,

$$\log u = 3{\cdot}134852 \quad t = 539061 \text{ years.}^*$$

$$\text{average velocity} = \frac{200000}{539061} = 0{\cdot}3710 \text{ yearly.}$$

To control this result, let us calculate also the velocity at an infinite distance, given by the formula,

$$v_0 = k\sqrt{\frac{e-1}{q}}$$

in our case 0·001006 daily, or 0·3672 yearly.

In order to allow of the comet's arrival 200 years sooner, it is sufficient that this velocity be increased by its $\frac{1}{2700}$th part, that is, by 0·000000372 unities daily, or by 0·66 metres a second.

As to the divergency of the fragments that arrived successively in 1677 and 1860, let us suppose the Sun to move yearly two unities through space, an estimation that probably is far too high. That body should have traversed, then, during these 180 years a distance of 360, which, seen from the star, represents an arc of 6′ sin 34° or 3′·6.

If, then, the comets of 1677 and 1860 were both fragments of the same body, it would have been sufficient for them to have left the star's sphere of attraction, removed 11 unities from each other, in directions diverging $3\frac{1}{2}$ minutes of angle, and with velocities the difference of which amounts to $\frac{2}{3}$ metre per second.

In the actual state of our knowledge there is therefore nothing absurd in admitting the first hypothesis.

* This result has a very great influence on the reasoning contained in § 5. In an interval of time comparable to such a number of years the Star with well-defined parallax of which there is question in § 5, may have had a very considerable proper motion along the sphere, and be removed far from the point it occupied when the comets started from it. Nevertheless I have preferred to retain that paragraph such as it was written in July, for the reasons mentioned in the note attached to it.

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As for the second, according to which we should have received bodies despatched at different times by a same star, it is a question of probabilities.

With a velocity such as probably was nearly that of the Comets of 1860 and 1863, the perihelion distance $q = 1{\cdot}3$ corresponds to such a direction of the initial movement as causes the body to pass the Sun at a distance of 27. When this latter number is doubled the perihelion distance becomes 5·2, that is to say, the comet is no more visible to the inhabitants of the Earth.

We may, therefore, compare the phenomenon to a firing at a target of 120 unities in diameter, at a distance of 206265, and in such circumstances that the person who fires is ignorant in what direction the target is to be met with. Its diameter corresponding to 2′, the probability is only $\frac{1}{4}\sin 2'60''$, that a second shot will strike the target already hit by the first. Firing at random we must discharge 47,300,000 shots in order to have the mathematical expectation 1 of producing the phenomenon.

To come back to our star, even if we knew that it scatters yearly 131,300 comets in space, even then we might *a priori* lay an even wager that the phenomenon of twice striking the solar target, as an effect of chance, will not occur within 180 years.

On the other hand, should there exist a physical cause which compels two successive shots to differ only $3\frac{1}{2}'$ in direction, and, supposing the target to have been already hit, we might lay 1 to 10 that it will be struck again by a second shot, and even 1 to 1 as soon as we know the divergency of the shots, and accordingly the target's motion, to be reduced to a $\frac{1}{8}$th part.

The admission of the second hypothesis includes therefore the supposition of a very great number of comets being yearly hurled into space by the star. Is there anything inadmissible in the number 130,000? If we suppose around the Sun, as a common centre, two spheres, whose radii are unity and *Neptune's* distance, there will be in the larger one a number of perihelia 27,000 times greater than that in the smaller sphere, supposing the perihelia to be uniformly distributed through space. This being so, the smaller sphere contains on an average 2 perihelia yearly, which gives 54,000 comets passing yearly through their perihelia within a sphere large enough to contain our planetary system. Let us add, first, that this number is doubled as soon as we admit that one half of the comets pass without being discovered; secondly, that it seems difficult to reject this uniform repetition of the perihelia, which makes their number proportional to the volume of the sphere, that is, to the cube of its radius.

Finally, neither of the two hypotheses leads us to the admission of anything absurd. It appears, therefore, premature for the present to give the preference to either of them.

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§ 7. Let us consider the two hypotheses from another point of view.

If the five comets are fragments of one body, they have been moving towards the Sun in directions diverging by no more than $3\frac{1}{2}'$, and which we may, therefore, consider as parallel to each other. In that case, the five orbits must have a single intersectional point.

On the contrary, if they have been despatched at different times, the intersectional point of the Comets 1677 and 1683 may differ from that belonging to the Comets of 1860 and 1863, and, in that case, a difference between these points of $\frac{1}{2}°$ or even $1°$ would not at all be astonishing, according to the contents of § 4 of my former paper.

Let us take the numbers from § 2. We have thus the intersectional points

of the Comets of 1677 and 1683, long. $= 318°{\cdot}5$, lat. $= -78°{\cdot}8$
,, 1863 I. and 1863 VI. long. $= 320°{\cdot}8$, lat. $= -78°{\cdot}7$

two points, whose mutual distance is nearly $\frac{1}{2}°$. It is difficult for the present to decide whether we must consider them as two distinct points, or whether we must simply attribute the difference to the lesser certainty of the ancient orbits.

A new computation of these, based on a new and careful reduction of the observations, with exact calculation of the planetary attractions, could alone give us the means of deciding.

§ 8. There remains still the Comet 1860 III., whose orbit passes at a distance of more than $1°{\cdot}5$ the average intersectional point of the four other orbits. The most simple supposition is that this comet has undergone some perturbation. Indeed, I find that, before passing its perihelion, it was near the planet *Mercury*, namely, at a distance of about 0·04. An approximate calculation, however, shows me that the attraction of this planet was insufficient to cause such a notable perturbation in the position of the orbit's plane, in the course of the three or four days during which the two bodies were near each other.

The phenomenon remains, then, unexplained. Has it had its origin near the Sun, or, long before that time, in space? In the latter case, the Comet 1860 III., on coming within the attraction of the Sun, must have had a movement the direction of which was convergent towards the parallel movements of the comets of 1863. May not the mutual attraction of these three bodies have exercised some influence of that kind during the course of several centuries?

§ 9. It is easy to denote their relative position, when at a great distance from the Sun.

First, we have for the mutual inclinations of their orbits,

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Inclination of the Orbit	1860 III.	on the Orbit	1863 I.	= 32° 29′
,,	,,	1860 III.	,,	1863 VI. = 20 27
,,	,,	1863 I.	,,	1863 VI. = 12 1

Further, for the perpendiculars drawn from the Sun on the tangents to the orbits (considered as parabolas), the formula

$$l = \sqrt{r\,q}$$

which gives

	Perpendicular.		
Gregorian Date.	Comet 1860 III.	Comet 1863 I.	Comet 1863 VI.
756·97	13·26	21·85	28 08
1020·87	12·10	19·94	25·63

so that we obtain—

	Date.	
Mutual Distances.	756·97	1020·87.
Comets 1860 III. and 1863 I.	12·81	11·71
Comets 1860 III. and 1863 VI.	16·31	14·90
Comets 1863 I. and 1863 VI.	8·10	7·32

If we prefer to consider the orbits as hyperbolas, we should have to calculate,

$$l = q\sqrt{\frac{1+e}{1-e}}$$

and, therefore, we must admit—

1. That the initial movement is the same for all, or that $\frac{e-1}{q}$ is a constant quantity in the three orbits.
2. That the excentricity of the orbit of 1860 III. has a certain value.

Let this quantity be again 1·001, we obtain then for the perpendiculars,

12·51 21·10 27·13

and for the mutual distances of the comets in space,

12·51 15·86 7·51.

numbers whose proportions do not considerably differ from the above given, and which, should it still be necessary, could give a new proof that before approaching the Sun our comets were near each other in space, moving in equal directions with equal velocities.

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Now it follows from these numbers that, if the comets have in space exercised some mutual attraction, we must in the first place seek for the effects of it in the Comets of 1863 which were always the nearest to each other.

No mutual attraction, therefore, explains the deviation of the orbit 1860 III. We might have recourse to the supposition of some meeting with an unknown body that may have more exclusively influenced this comet, but it would be explaining the unknown by the unknown, and that is a way in which I do not like to venture.

§ 10. There is still another circumstance worthy of being mentioned, because it may perhaps lead to a distinction between the two hypotheses of § 6. I mean the distribution of the aphelia around the intersectional points. If we follow the orbits in the direction of the motions of the comets, we meet, in those of 1860 and 1863, with the aphelia before reaching the points of intersection. The contrary is the case with those of 1677 and 1683.

I do not doubt that there is an intimate connection between the excentricity of each orbit and the position of its aphelion with reference to that of the point of intersection. Further, we have a well-known relation between its excentricity and the velocity of its initial movement.

But it seems that any conclusion is premature, before a new computation of the orbits has been made, with an investigation, for each of them, of the maximum as well as the minimum of excentricity, that the observations allow of.

§ 11. Let us return for a moment to the differential formula of § 4.

Neglecting the small quantity q, as we may do in comparison to large values of r, we obtain,

$$dt = \frac{3\,C}{2}\sqrt{r}\,dr$$

a formula that enables us to deduce the following result:—If we admit that, at a distance of 600 unities from the Sun, the different members of a cometary system may have outrun each other by 10 unities, in consequence of a small difference in their respective velocities, it is possible that three comets, formerly united in a system, may pass through their perihelia during a course of 55 years.

A new combination of our whole stock of aphelion-positions is, therefore, necessary with adequate enlargement of the limit of time, which has been chosen too small in § 1.

I intend to make the investigation.

Utrecht, 8 *July*, 1865.

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On the great Sun-spot of October 1865.

By the Rev. F. Howlett.

Many telescopes were, I hope, directed to the remarkable spot which forms the subject of the diagrams now suspended before you.*

The features which this spot exhibited during its development were, I conceive, of rare interest, whilst the definition which prevailed at the time was everything that could be desired.

It was first observed by me on the morning of the 7th Oct. last, at 7 A.M., and when not more than about 3″ from the Sun's eastern margin (fig. 1). It then appeared about 57″ in length from N. to S.; but, in consequence of the extreme effects of foreshortening on the surface of the sphere, only 3″ in breadth from E. to W.

Narrow though the spot, therefore, at this time appeared, yet the positions of the umbræ within it were distinctly visible, and distinguishable from the penumbra in directions of latitude, though I was unable to discern clearly between umbra and penumbra in directions of longitude.

One feature which it struck me as certainly not often to be seen at such an *early* stage of a spot's entry on the disk, was the appearance of two or three luminous patches of faculæ (immediately adjoining the spot on its following side), which extended *completely up to the very margin* of the Sun. It is seldom, I mean, that any difference of luminosity can be perceived in the Solar photosphere, say, within 10″ to 15″ from the limb; though, where faculæ or other inequalities of luminosity exist, they are visible enough when further removed from the margin, their utmost apparent brilliancy manifesting itself, generally, when they are from 20″ to 2′ from it, or thereabouts. By $2^h\ 15^m$ P.M. on the same day (Oct. 7th) the apparent mean breadth of the spot had attained about 7″, and the distance from the limb to the centre of the principal (or western) umbra was 13″ (fig. 2).

The distinction between umbra and penumbra was now in all parts discernible; and I had at this hour more satisfactory evidence of the *shelving* nature of the sides of a Solar spot than I think ever occurred to me before; for the penumbra on the *following* or *outer* side of the umbra was plainly broader by about 3″ than it was on the preceding or inner side, on which last, in fact, it could not have been much more than 1″ in apparent breadth.

So very shallow, however, is the amount always of this shelving, at least in the penumbral strata, and so seldom, comparatively, have I enjoyed the opportunity of observing the entry or departure of a really good-sized spot, furnished at

* These diagrams were exhibited at the Meeting. *Ed.*

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the same time with a neat and really *centrally situated* umbra, that it has only been on two or three occasions, during my six years' experience, that I can fairly assert that I have seen this difference of fore-shortening at all. And I cannot but think that some of the supposed instances of the kind have been sometimes fallacious, and that the spots under observation were not really furnished with centrally placed umbræ, and not therefore proper subjects for so delicate an investigation.

On the morning of Oct. 8th the spot had attained a length of about 76″ by an apparent breadth of about 16″. Its centre was now about 52″ from the Sun's limb; and the penumbra appeared equally wide on both the preceding and following sides of the principal umbra, which lay within the southern portion of the spot (see fig. 3).

My observations, however, for 8th Oct. were rather hurried, and I am somewhat inclined to question whether there was really such an absence of umbra in the northern portion, and I should wish my opinion to be checked by that of any other observer.

My notes and drawings for the 10th Oct. are, I trust, as correct as need reasonably be required, so far as the instrument at my command is concerned, aided, moreover, by an atmosphere of unusual serenity (fig. 4).

The centre of the spot (which, it will now be seen, was the leading one of a group) was at 9 A.M. just about 4′ from the limb. The penumbra had a length of about 80″ by a breadth of 50″. The umbra, now of a ragged rectangular form, was situated as nearly as possible in the centre of the spot. It had a mean length of 27″ by a mean breadth of 17″. Two long promontories of different degrees of luminosity extended far across the chasm. The brighter one, 7″ in length (or 3150 miles) from the southern side; the other about 8″ (or 3600 miles) from the northern. As Mr. Brodie observed in a brief notice of this spot to the *Times* newspaper, the shorter and brighter promontory afterwards was separated, and moved away from the penumbra; and the commencement of this operation is plainly visible in my drawing for this day. Not only was the southern promontory the brighter one by far of the two just described, but nearly the whole of the southern side of the umbra was bordered by a luminous band of considerably greater brightness than the penumbral regions immediately adjoining. At the same time, the darkness of tint of the eastern side of the umbra itself was far deeper than that of the western, over which, especially in its north-west angle, floated a feeble luminous haze.

As may be seen in the drawing, the penumbra itself was far from possessing one uniform tint; but, as is often the case, was considerably varied by specks and bands.

In a note for Oct. 11th I have entered the following statement:—"Too stormy to obtain a drawing this day; but about

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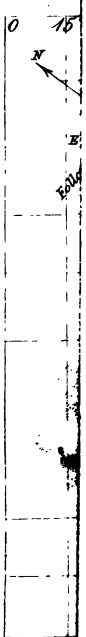

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noon the remarkably luminous band, which bordered the umbra on its south-eastern side, had completely detached itself from the penumbra, and had assumed the appearance of a narrow tortuous 'bridge,' nearly conformable to the wavy edge of the same side of the penumbra; ramifications of feebly luminous matter extending into the northern portions of the penumbra."

On October 12th the great spot had assumed a very different appearance (fig. 5), partly, perhaps, in consequence of the almost entire absence now of the effects of fore-shortening, but even yet more in consequence of the umbra having undoubtedly extended itself much more in an easterly and westerly direction. The great leading spot was now 90″ in its most extreme length by 60″ in mean width. From the north-west side of the umbra projected a long, narrow sigmatoid promontory, somewhat brighter than the adjoining penumbra, about 7000 miles long and 1000 miles wide. Other broader and shorter promontories also stretched into the umbra, whilst a somewhat crescent-shaped and rather less luminous cloud floated as nearly as possible over its centre; some very feebly illuminated matter extending itself in hazy masses 10,000 miles in length over its south-east portions. One or two bright oval patches, from 2000 to 2500 miles in length, lay imbedded in the penumbra, where it was of a somewhat darker hue than the average tint.

By 4 P.M. the sigmatoid promontory had completely detached itself from the penumbra, and had assumed a simple crescent form, *convex* towards the west. Indeed, about one-third of its length at its south-east extremity had disappeared; hence its now crescent shape. Meanwhile, one of the broad southern promontories had nearly united itself to one of the northern ones, curving towards the north-east as it did so. The edge of the penumbra generally was characterised by indentations of considerable depth; but it was still more ragged on the day following.

This is shown in the drawing for Oct. 13th, and is more especially to be observed along its southern border (fig. 6). The remaining outlying and following spots of the group were now arranged after the fashion of a rude triangle, each of whose sides was about a minute and a half in length.

The group this day had attained its most central position on the disk; and as regards size, also, had now reached its utmost dimensions, being about 110″ in length by 60″ still in breadth; and, making every allowance for its oval and also somewhat irregular contour, must have had a superficial area of certainly not less than 972,000,000 square miles. The remaining small spots had a joint area of about 165,000,000 miles; making a grand total of displacement of the solar photosphere to the enormous extent of 1,137,000,000 miles square, or nearly six times that of the whole surface of the earth!

The penumbra on the morning of the 13th was marked by

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a long dark streak in its northern portion, about 40″ in length and only about 2″ in breadth, which by noon had become more dark and distinct, as if about to become a narrow umbral *rift;* and other shorter streaks lay in nearly parallel lines with it, towards the south-east; whilst another streak from its north-west extremity ran at right-angles into the northern side of the umbra, and was divided across by a small bright patch at the hour last mentioned; by which time also a large square projecting portion of penumbral matter had nearly detached itself from the main spot towards the south-east.

But the most remarkable feature, perhaps, this day was a bright, sharply-defined arch of photospheric matter, about 9450 miles long in its total curve, which floated over the western side of the umbra, and was united at each extremity to the northern side of the penumbra.

I was now also able, as I thought, to discern four separate nuclei in different parts of the great elongated umbra, which feature itself was not less than 26,000 miles in length by 15,000 in breadth; and wherein, towards the south-east, a large triangular mass of the feebly luminous haze was still plainly observable. It is, perhaps, possible that the arch above described was constructed by the union of one of the promontories with the remains of the sigmatoid extension which existed, as we have seen, on Oct. 12th.

By 8 A.M. on Oct. 14th the great spot presented a very different appearance (fig. 7). The umbra was completely divided across into two unequal portions by an exceedingly bright and nearly straight bridge, 9000 miles long and 1000 miles wide, as before, and which seemed to be formed in part, possibly, out of a modification of the north-west remains of the arch of the day previous. It lay exactly over the principal nucleus of the great umbra, and was considerably dilated at its northern foot. But whether it was that the arch had swung itself loose at its north-east extremity, and then subsequently stretched itself out across the whole width of the umbra, or whether it was an entirely new extension of photospheric matter, I was unable to determine; but subsequent observations have led me to suspect rather the former.

The eastern, and as yet the larger portion of the umbra was now considerably contracted, as regarded its breadth, whilst at the same time the long narrow rift had become more decidedly dark and umbral in its character, in many places. The other streaks which, as we observed above, lay in directions nearly parallel with the rift on the 13th Oct., were now replaced by others, and which, in direct connexion with it, ran off from it into the main umbra, at an angle of somewhere about 45°.

By 2 P.M. a very narrow streak of bright luminous matter extended itself from the northern extremity of the bridge all along the southern edge of the rift,—symptoms of which

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formation had manifested themselves as early as 9 A.M. The rift, moreover, was divided by the luminous matter in two or three places, previously to its gradual closing up, as it afterwards proved. Equally curious were certain faint but perhaps not the less important features observable at this time amongst the small subordinate spots that followed in the wake of the principal one. These were evidently diminishing in magnitude, but in their manner of closing up they seemed to betray a noteworthy sort of movement amongst themselves, or perhaps rather, in the first instance, in the photosphere in which they lay scattered. The largest of these minor spots (which was of the dimensions of about 20″ by 15″) had either drifted away from, or had been left behind by, its principal; whilst at the same time a very peculiar sort of *trailing* arrangement was being assumed amongst themselves, indicating a kind of gyratory movement in the photosphere itself. Many of the little spots had ceased to exhibit any appearance of umbra, but, on the other hand, they had become united more intimately one with the other, by means of a wavy and here and there divaricating thread of penumbral specks running through them; an arrangement this, which may also be distinctly observed in my records, for instance, of 11 May, 1863, and of 1 Feb. 1864.

On the 15th Oct. I was unable to make a detailed drawing of the group, but I observed distinctly that the great bridge had again become thoroughly curvilinear in its disposition (fig. 8), extending also further up towards the western end of the umbra, and having a total length of a clear 30″ or 13,500 miles. Indeed, in its now modified condition, and as being almost separated, in its new north-eastern portions, from the adjoining penumbra, it might almost be stated as being even 48″, or 21,600 miles long. The rift was also still plainly to be distinguished, but its more easterly portions had nearly closed up.

On the morning of Oct. 16th the great spot — (or craters, as these phenomena have been called by M. Chacornac, and which term, as Mr. Brodie observed to me, would seem to be a far more appropriate designation for these mighty gulfs in the solar surface) — presented a truly marvellous and most interesting appearance. The great western bridge was now rent entirely asunder. Its northerly half, which remained still connected in that direction with the penumbra, had again become nearly *straight*, and terminated abruptly just above the principal nucleus of the great umbra; whilst its other portion appeared actually to have swung itself round, somewhat after the fashion of a prodigious flying-bridge (*pont-volant*); and, remaining still attached by its extremity to the penumbra, as before, had thrown itself into the form of a huge brilliant *loop;* being exactly a counterpart, in fact, of the curious arch of Oct. 13th; only it now lay on the southern side of the umbra instead of the northern.

As regarded meanwhile the promontories generally, some

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of them (especially the most westerly ones) appeared to be obeying some powerful cyclonic impulse, the vortex of which might be assigned probably to the principal nucleus alluded to above. But if so, the impulse in question seemed to be manifesting itself at a lower level than that of the broken luminous bridge, and also of the loop; which last feature had, by noon, this day, very nearly indeed reached to the northern side of the umbra.

The other promontories seemed mostly to have one general tendency of direction towards the west; and the same causes which regulated their shapes and bearings may be seen in the drawings to have for some time past operated conspicuously upon the disposition of the rest of the penumbral matter as well. The subordinate or following spots of the group continued now rapidly to diminish more and more.

Oct. 17th was the last day on which I was enabled to obtain a view of the group, and that was but a comparatively transient one (fig. 10). But the principal spot still appeared about 95″ in length by 65″ in breadth. The large umbra (as I suspected would soon be the case) was now completely divided into two portions by a broad mass of intervening penumbra, dotted in three or four places by dusky patches, indicating the region formerly occupied by the central portions of the one single umbra. At the eastern border of the lately formed penumbra just mentioned a small luminous loop was still observable; and which I rather think was the diminished representative of the striking feature of the previous day, though of this I do not feel quite certain.

At 8 A.M. on the morning of Oct. 20th I observed a somewhat conspicuous and condensed mass of faculæ exceedingly close to the Sun's western limb, and in a latitude which must have very nearly coincided with that occupied by the notable group which had just passed off; and of which, indeed, it was, probably, for the time, the last indications.

On and after Oct. 20th the Sun's disk continued absolutely devoid of spots for fourteen days, till 3d Nov., when one, about 35″ in length, possessing two umbræ, and attended by a fine display of faculæ, made its appearance in nearly precisely the same latitude as that wherein lay the spot which has formed the subject of my paper this evening; and we have, therefore, little room for doubting but what it is the same interesting visitant again come round, and seeking further acquaintance.

I have two drawings of it, on my ordinary scale of one inch to thirty seconds; and in the second of which the faculæ are unusually distinct, and in part, again, coming up absolutely to the very margin of the disk.

With regard, finally, to the prolonged absence of spots alluded to above, I would observe, in conclusion, that I have not known the Sun to be totally devoid of spots for the same

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number of days (in the aggregate even) during the last six years; and about one-third of those previous blank days have occurred, be it noted, during about the last six months.

Lastly, then, from all the appearances which have been presented by this remarkable spot we may clearly again infer the fallacy of such theories as would attribute a more or less solid and simply opaque nature to these wonderful phenomena (*especially the darker portions*); be it either that of a cloudy condensation floating above the photosphere, according to some physicists, or of actual crystallization or other form of more decided solidification lying upon it, according to others.

Or how can we suppose that the brilliant and mobile phenomena exhibited by the constantly shifting loops, bridges, promontories, and patches of feebly luminous haze can be rents merely in, or more or less porous portions of, the inspissated matter, disclosing more or less distinctly the subjacent solar photosphere? Would not any such rents, if they were such, prefer to run their course through the less solid portions of the mass—through the penumbræ, for instance, instead of the umbræ chiefly, or, more strangely still, through the nucleus even? But yet, in figs. 5, 6, 7, 8, and 9, it may be observed how the intensely luminous promontory, loop, or bridge (as the case may be), cuts completely through the very *nucleus* itself of the spot: an almost incredible circumstance this, supposing the spots to be solidifications of matter; but perfectly reconcileable with the Herschelian theory (which, in general, is that of nearly all English astronomers who have studied carefully solar phenomena, as well as that of MM. Schwabe, Chacornac, Secchi, and other able continental observers), which assumes, in perfect accordance with the ocular evidence afforded also by the best and most recent methods of scrutinising the solar surface, that the outer portions of the solar orb consist of various strata or envelopes of incandescent liquid or gaseous matter (and in which, possibly, may float particles of a more decidedly solid nature) of different degrees of luminosity, the uppermost being the brightest, and affected by certain most powerful motive forces, the true nature of which is yet enveloped in much obscurity, but which would appear to be, at times, partly cyclonic and perhaps partly also magnetic in their character; and, finally, that the so-called spots or craters are undoubtedly *negative* (that is, cavernous), and not *positive* (that is, concretionary), in their character.

The President said, You will agree with me as to the value of these continuous observations of Mr. Howlett. Unfortunately, members who do not attend our meetings have not the opportunity of seeing how very beautiful are the representa-

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tions of Sun-spots, for which we are indebted to Mr. Howlett. It is only by continuity in observations that we can hope to lay the foundation of a theory which will account for all the phenomena of Solar Physics. And while for the time we may accept as useful any theory which will embrace certain phenomena, it should be laid aside as soon as a more comprehensive one is propounded. It is very certain that our business is to multiply accurate observations, and a good result will follow. One thing seems fairly established, namely, that there are several layers of matters in the photosphere, but the relative positions of these strata is still an open question. In a paper now passing through the press by Mr. Stewart, Mr. Loewy, and myself, I think some light will be thrown on this disputed point.

Some Observations on the Solar Craters which appeared on September 28 *and October* 8, 1865. By H. Brodie, Esq.

On September 28 a very fine crater was observed to be on the Sun's disk, the length of the umbra being about 9200 miles. The penumbra was deeply indented at the bottom, where it met the umbra, and one of the promontories formed by the indentations of the penumbra projected much further than the rest; the end of this one was observed to break off in the shape of a roundish mass of luminous matter, having a diameter about equal to the width of the promontory, from which it separated, this nodule of matter was not visible about half an hour later, but seemed to have been diffused on the surface of the umbra, which was covered with a wispy stratum of thin luminous matter, except at one place about the centre of the umbra, where it appeared black, forming the so-called nucleus of the crater. On one side of the umbra a very unusual appearance was noticed; the umbra seemed covered with this luminous matter in the form of what is commonly called mackerel sky, or "cirro-cumulus," the nodular portions of which became smaller as they advanced on to the umbra, and at last diffused into the wispy stratum before mentioned.

September 30. The umbra similar in appearance to that of the 28th inst., the whole being maculated with luminous matter, except the nucleus; these spots of luminous matter being much softened or diffused at their edges. Many nodular portions of luminous matter had broken off from the toothed formation of the penumbra, and were drifting towards the centre of the umbra, but were soon diffused into the thin luminous stratum covering the umbra generally. Some of these toothings, or promontories, were 2″ of arc long. The penumbra was apparently deeply furrowed from the top edge, or Sun's

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surface, to the bottom in tolerably straight and continuous channels, very similar to deep-water courses seen on the sides of a mountain. From watching the effect of these changes it would appear that portions of the photosphere are continually breaking away and sliding down the sides of the penumbra, then breaking off into small round portions and drifting on to the umbra, where they become diffused into a thin luminous stratum; and since this stratum does not become more dense, it must be presumed that this luminous matter forming the Sun's surface is absorbed by chemical actions as yet unknown to us.

October 2. On examining the position of the crater to-day I find that there has been an axial rotation of about 33½° since September 30. This crater disappeared from the Sun's disk about October 5.

October 8. Found a very large crater coming on to the Sun's disk, but too close to edge of disk to examine it well.

October 10. On taking some micrometrical measurements of the crater, and allowing for the foreshortening due to its position on the Sun's sphere, the true shape of the crater appears to be tolerably circular, the umbra having a mean diameter of about 15,000 miles and the penumbra having a mean diameter of about 38,000 miles. There were two long promontories of luminous matter stretching on to the umbra, and nearly opposite to each other, one on each side of the umbra; of these one measured about 3000 miles in length, the other about 4200 miles in length. Soon afterwards the end of the shorter promontory broke off and drifted on to the umbra; about three hours later the whole of this promontory had detached itself from the penumbra, and was being dissipated into thin luminous cloudy stratum.

October 11. The definition this morning is finer than I have yet seen it; used a positive power of 470, with a Dawes' solar eye-piece, and got very sharp definition. The prevalence of passing clouds rendered it impossible to observe continuously. The length of the umbra to-day was about 18,000 miles, the width about 9700 miles. There was an exceedingly long promontory of luminous matter projecting along the umbra from one end of the crater, and running tolerably parallel to one of the sides, and not far from the end of this promontory was another detached mass of luminous matter, about 4000 miles long. (See sketch No. 1.*) This latter mass had elongated itself with wonderful rapidity during the preceding 15 minutes, possibly as much as one quarter of its length. The umbra was covered with the same sort of maculated, or "mackerel-sky" formation of thin luminous stratum, as noticed in the crater of September 28, especially on the opposite side to that near which the promontories of luminous

* The several sketches referred to were exhibited at the Meeting. *Ed.*

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matter were located. The very long promontory seemed to be drifting towards the edge of the umbra, while the detached mass was moving away from the edge, and the two masses being so contiguous seemed to argue the existence of a cyclonic action of the disturbing causes.

About 1½ hours later I found that the detached mass had formed a junction with the long promontory (see sketch No. 2), and near the end of this now doubly long promontory there was a curious network sort of formation, consisting of lines of thin luminous cloudy stratum; apparently these had formed by being condensed from the surrounding thin luminous stratum. The prevalence of passing clouds prevented my watching these changes.

October 12. The shape of the umbra very greatly altered, its greatest length this morning was nearly 29,000 miles, while the greatest length of the penumbra was rather more than 50,000 miles. The long promontory of yesterday had entirely disappeared, but there was one at the opposite end of the crater shaped something like an S. (See sketch No. 3.) The singular changes which took place in connexion with this serpentine promontory are shown in sketches No. 3, 4, 5, and 6. The intervals of time being indicated, the outline of the umbra in these last sketches was taken from the image of the crater being thrown by the telescope upon a board. I noticed at one time five nodules of luminous matter that had broken off from the toothings of the penumbra, all in a row drifting towards the middle of the umbra, these were soon after diffused into thin cloudy stratum.

October 13. The sketch No. 7 shows another change from No. 6 that was taken yesterday; the bridge in No. 6 being entirely broken away, and a second umbra is now breaking through the penumbra.

These observations were made with an equatoreal of 8½-in. aperture and 11½ feet focal length.

Molesey Gore, Uckfield, Sussex.

The President desired to make one remark in reference to Mr. Brodie's paper, that one of his observations confirms those of many others, namely, that the luminous matter as it floats across a spot seems to dissolve and disappear.

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Some Remarks on the Solar Photosphere.

By Isaac Fletcher, Esq.

Since the date of my communication to the Society on the photosphere of the Sun, printed in the June number of the *Monthly Notices*, I have on very many favourable occasions examined with every possible care and precaution the visible surface of the Sun, employing the same optical means, viz., my 9½-inch refractor, with a Dawes' solar eye-piece and powers from 100 to 550. The result has been a strong confirmation of the opinion I have long held, that recent telescopic observation has (with the single exception of Mr. Dawes' remarkable discovery of the *cloudy stratum*) thrown little, if any, additional light on what Sir William Herschel aptly termed the "Nature of the Sun." Nor, when we reflect on the circumstances of the case, is this to be wondered at, for when that distinguished man observed the Sun he knew better how to proceed to counteract the intense heat of the solar focal point, than to contract the aperture of his telescope, as has been the fashion (until the last few years) in more modern times. In observing the Sun, Sir William placed between the small mirror of his Newtonian reflector and the eye-piece a rectangular vessel with well-polished and parallel glass plates at opposite sides, through which the rays were transmitted on their passage to the eye-piece. By filling this vessel with water, diluted with ink or other coloured liquid, he was able to reduce the light and heat of the solar beam to any amount desired before it was received by the eye, without any contraction of the aperture of the telescope.

Under date, May 3, 1801, using a solution of ink, he says, "Through this mixture I can observe the Sun in the meridian, for any length of time, without danger to the eye or to the glasses, with a mirror of nine inches in diameter, and with the eye-pieces open, as they are used for night observations." (That is without coloured glasses.) With this apparatus there can be no doubt he obtained views of the solar surface equal in every respect to those more recently obtained by means of Sun prisms or other glass diagonals, and his description of the various appearances presented to him corresponds exceedingly well with what I am able to see with my large refractor. I am referring now to the general surface of the Sun, and not to the phenomena visible in the penumbræ of spots. In Mr. Dawes' most interesting paper on this subject, read before the Society, Jan. 8th, 1864, he gives various extracts from Sir W. Herschel's first paper on the "Nature of the Sun," corroborative of this view of the subject; but in Sir William's second paper, printed in the same volume of the *Phil. Trans.* (1801), there are various observations recorded which seem to me conclusive as to the fact of his being perfectly familiar

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with the "granules" which have arrested the attention of recent observers, and which some have erroneously supposed to be identical with the "willow-leaf" structure figured by Mr. Nasmyth. I append some extracts from the paper I have alluded to, premising that, beyond all doubt Sir William's "corrugations" are the "rice-grains," "shingle-beach," "minute fragments of porcelain," and "granules" of recent years.

"March 18, 1801. The corrugations all over the Sun are beautiful and coarse, resembling small nodules joined together like irregular honeycomb.

"In a multitude of places the corrugations are quite detached, like luminous wisps, or slender tufts, standing upright.

"March 19. The corrugations are rich, and may be called luminous wisps, being much disjointed except at their bottom; they are so rich that they partake of the yellowish colour of the ridges.

"March 21. At equal distances from the limb the corrugations are equally coarse all over the disk of the Sun.

"March 31. An opening very near the preceding limb is surrounded by a shallow, which is bordered by a luminous ridge all round it. The opening itself is also bordered by an elevated edge, which is nearly as high as the general surface of the corrugations; but not so high as that which borders the shallow, and stands above the general surface.

"April 1. The Sun is now without any openings; but the corrugations are very luminous and rich.

"April 2. The Sun is very rich in luminous corrugations, interspersed with bright nodules towards the south pole.

"April 10. The Sun is full of rich tufted corrugations.

"April 19. The corrugations are extremely rich. The whole solar surface seems to be studded with nodules.

"April 20. The whole surface of the Sun is rich; the corrugations are tufted.

"April 24. The corrugations seem to be closer than they were yesterday.

"April 29. I viewed the Sun through a mixture of ink diluted with water, and filtered through paper. It gave an image as white as snow; and I saw objects very distinctly, without darkening glasses.

"The ridges through this composition appear whiter than the rest of the Sun.

"The tops of the corrugations are whiter than their indentations, instead of approaching to a yellowish cast, as they do in my former way of seeing through green smoked glasses.

"The corrugations are very small and contracted to-day."

From these observations and others in Sir William Herschel's first paper which Mr. Dawes has laid before the Society, and a reference to his drawings, I think that no reasonable doubt can remain on the mind of any dispassionate inquirer, that Sir William's corrugations are identical with the appear-

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ances which, under various names, have recently attracted so much attention.

I think it highly probable that from the date of Sir W. Herschel's observations to within the last few years, the Sun has rarely been looked at except with apertures contracted to 2 or $2\frac{1}{2}$ inches, and, under such conditions, the wonderful appearances so graphically described by him are not to be discerned at all, as I have ascertained by direct experiment.

According to the observations of Mr. Nasmyth, the entire photosphere of the Sun is composed of multitudes of lenticular objects, exceedingly regular in form and dimensions, crossing and overlaying each other in every possible direction, and the mottled appearance of the Sun is due to the interstices caused by such interlacing. My observations taken with, I believe, one of the finest telescopes in this country, and under excellent circumstances, not only fail to support Mr. Nasmyth's conclusions, but give direct and strong evidence to the contrary. My conclusion is entirely in accordance with that of Sir W. Herschel and Mr. Dawes; viz., that the "corrugations" or "granules" are portions of the general surface of the photosphere raised high in the outer and non-luminous atmosphere possibly by the furious escape of empyreal gas from the regions beneath the photosphere. I have never seen any appearance whatever indicative of a structure such as is described by Mr. Nasmyth, except what was evidently caused for the moment only by atmospheric disturbance. Nevertheless, I think there is evidence that the photosphere does consist of masses of luminous cloud of some kind, but probably of a substance (so to speak) much more dense and solid than our terrestrial clouds. This, I think, is shown by the manner in which the luminous masses are drawn out and attenuated on the penumbra of the large spots. Here they generally present (though with occasional irregularities, as shown in the beautiful drawings of Mr. De La Rue and Dr. Müller) a perfectly radial appearance, such as one might suppose would be occasioned by a downward rush of the luminous matter on the sudden removal of support from beneath. In such a case the denser portions of the luminous matter and the less dense, would probably so arrange themselves as to produce the beautiful radial appearance in question, whilst lateral disturbance would give the leafy or mossy appearance figured by our President and Dr. Müller.

An apparently fatal objection to the idea of a *downward* rush of the luminous matter is the fact which every observer has noticed, that there is an evident upheaving of the photosphere round the penumbræ of large spots. It seems, therefore, more probable that the appearances on the penumbræ are caused by an *upward* rush of gaseous matter, a theory which, I think, will explain the different appearances surrounding the nuclei of Spots.

After all, it must be confessed that as yet little progress

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has been made towards a solution of the query, "What is a Sun?"

Turnbank Observatory, Cumberland, Nov. 6, 1865.

Mr. Stone: I think in some of his latter letters Mr. Dawes refers to two distinct cloudy strata which he observed on the the solar photosphere, one of which he designates as having coarse and the other finer granulations. I am inclined to think that the coarser of those are identical with the "corrugations" of Herschel. It would appear therefore that Herschel overlooked some of the appearances since described by Mr. Dawes, possibly because his attention was attracted by other phenomena, for example, the dark interstices which he describes as existing between the brighter portions of the photosphere.

The Astronomer Royal suggested that possibly the definition of Herschel's telescope was not so good as those of modern times.

Mr. Brodie: There may be a difference of opinion as to what Herschel meant by corrugations, he may have meant the so-called granules; but there is another very definite feature with large instruments, namely, mountainous ridges—almost like faculæ over the whole of the Sun's surface; these upheavals leaving valleys 60″ or 70″ broad. These ridges of luminous matter are a separate formation from the small granulated appearance of the Sun, and it is possible that Herschel's corrugations are the ridges.

Mr. Stone: I think he refers to these specially under the term of ridges.

Mr. Fletcher: The coarse granules on the Sun may be seen with almost any telescope: one of two inches in aperture will show them distinctly. But it requires a much larger one to see the finer granules on the Sun's disk.

Mr. Stone: May I ask you whether you tried to estimate in any way the dimensions of these granules?

Mr. Fletcher: I think from 2″ to 3″ or 4″.

Mr. Stone: They appear to us to be much smaller.

The President: Certainly they are much larger than the markings which I considered to be Nasmyth's willow-leaves.

Note on an Error of Expression in two Memoirs of the Astronomer Royal, in the Corrections to the Elements of the Moon's Orbit. By the Astronomer Royal.

In a Memoir published in the xviith volume of the *Memoirs* of this Society, page 53, I have used the expression "the

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inclination [of the Moon's Orbit to the Ecliptic], whose assumed value is $18537''\cdot9$, is to be diminished by $2''\cdot44$, and its value is therefore to be reduced to $18535''\cdot46$." And in a second Memoir, published in the xxixth volume, page 21, the same form of expression is used, giving $18535''\cdot55$ for the inclination.

The words here used are incorrect, as will be seen on reference to the *Reduction of Greenwich Lunar Observations*, vol. i. page lxxix., of which these Memoirs are a sequel. The number $18537''\cdot9$ is the coefficient (slightly altered from Damoiseau's $18539''\cdot8$) of the first term in the development of the Moon's latitude in periodic functions of the true argument of latitude, and is *not* the inclination. The same remark applies to the numbers $18535''\cdot46$ and $18535''\cdot55$.

The small corrections made to the coefficient of the first term in the development of latitude correspond without sensible error to the corrections of the assumed inclination.

I am indebted to the careful examination of M. Delaunay, and to his courteous communication, for the discovery of this error.

Royal Observatory, Greenwich, 1865, *Oct.* 23.

On Lambert's Theorem for Elliptic Motion.

By J. J. Sylvester, Esq., F.R.S.

The original demonstration by Lambert of the celebrated theorem which bears his name was a geometrical one, see *Monthly Notices*, vol. xxii., p. 238, where this demonstration is reproduced by Mr. Cayley. Lagrange has given no less than three distinct demonstrations of the same: one a sort of verification by aid of trigonometrical formulæ, another founded on a property of integrals, and a third, perhaps the most remarkable of all, derived from the general expressions for the time in an orbit described about two centres of force varying according to the law of nature by supposing one of them to be situated in the orbit itself, and to become zero. Notwithstandng this plethora of demonstration, the following direct algebraical method of proving from the ordinary formulæ for the time of a planet passing from one point to another, that, when the period is given, the time is a function only of the sum of the distances of these points from the centre of force, and of their distance from one another, may be deemed not wholly undeserving of notice.

Let ϱ, ϱ' be the distances of the two positions from the Sun, c their distance from one another, v, v' the true, u, u' the excentric, m, m' the mean anomalies thereunto corresponding, e the excentricity, $\omega = m - m'$, $s = \varrho + \varrho'$, $\Delta = \frac{1}{2}(s^2 - c^2)$: then

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$$\varrho = 1 - e\cos u,\ \varrho' = 1 - e\cos u',\ m = u - e\sin u,\ m' = u' - e\sin u',$$

$$\varrho\cos v = \cos u - e,\quad \varrho\sin v = \sqrt{1-e^2}\sin u,$$

$$\varrho'\cos v' = \cos u' - e,\quad \varrho'\sin v' = \sqrt{1-e^2}\sin u',$$

$$c^2 = \varrho^2 + \varrho'^2 - 2\varrho\varrho'\cos(v'-v).$$

Writing for brevity c, c', s, s', for $\cos u$, $\cos u'$, $\sin u$, $\sin u'$, and to avoid confusion putting also for the moment $\bar{s}$ $\bar{c}$ in place of the original s and c, we have

$$\bar{s} = 2 - ec - ec',\quad \omega = u - u' - es + es',$$

$$\Delta = \varrho\varrho' + \varrho\varrho'\cos(v'-v) = 1 + cc' + ss' - 2e(c+c') + e^2(1 - cc' - ss').$$

Let $J = \frac{d(\Delta, \bar{s}, \omega)}{d(e, u, u')}$; then J is the determinant

$$\begin{vmatrix} -2(c+c') + 2e(1+cc'-ss'); & c's - cs' + 2es - e^2(cs' + c's); & cs' - c's + 2es' - e^2(cs' + c's) \\ -c - c' \quad ; & es \quad ; & es' \\ -s + s' \quad ; & 1 - ec \quad ; & -1 + ec' \end{vmatrix}$$

Denoting this determinant by $\begin{vmatrix} A, & B, & C \\ D, & E, & F \\ G, & H, & K \end{vmatrix}$, we find

$$(A, B, C) - 2H(D, E, F) + 2E(G, H, K) = (o, B, -B),$$
$$(A, B, C) - 2K(E, D, F) + 2F(G, H, H) = (o, -C, C),$$

so that $J = \begin{vmatrix} A, & B, & C \\ o, & B, & -B \\ o, & -C, & C \end{vmatrix} = o.$

Hence restoring s, c, instead of $\bar{s}$, $\bar{c}$, it appears that $d\omega$ is a linear function of ds and $d\nabla$; that is, ω is a function of s and ∇, or what is the same thing of s and c, independent of e. If then, when $e = 1$, the corresponding values of ϱ, ϱ', v, v', u, u' are r, r', θ, θ', ϕ, ϕ', we have $\cos\theta = -1$, $\cos\theta' = -1$, $\sin\theta = o$, $\sin\theta' = o$, $r - r' = c$, $r + r' = s$, whence writing

$$1 - \cos\phi = \frac{s+c}{2},\quad 1 - \cos\phi' = \frac{s-c}{2},$$

we have finally $\omega = \phi - \phi' - \sin\phi + \sin\phi'$ as was to be proved.

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Essentially this demonstration is of the same value as the first of Lagrange's three methods of proof above referred to, but with the difference that it leads up to and accounts beforehand for the success of the transformations therein employed.

Observations of Encke's Comet. By John Tebbutt, jun., Esq.

I succeeded in discovering Encke's Comet on the evening of the 24th June, with the help of a rough calculation founded on the theoretical elements in No. 1326 of the *Astronomische Nachrichten*, and assuming its perihelion passage to occur on the 1st June. I found it after a short search, but at a considerable distance from its calculated place. It was about two minutes in diameter, faint, and without the slighest condensation of light in the centre. Owing to an unfortunate detention of the mail-steamer, Mr. Farley's Ephemeris did not come to hand till the 30th June. Notice of the discovery was at once forwarded to the Sydney Observatory, and Mr. Smalley succeeded in observing the comet with the 7-inch refractor on the 28th June, and three following days. Owing to bad weather and bright moonlight, Mr. Ellery had not seen the comet up to the 7th instant. It was re-discovered here after the full moon, with the assistance of the ephemeris, but it was so faint as to be hardly distinguishable. The two determinations of position which I send are the best obtained, and may possibly be of some value. I hope to be more fortunate in the observation of Biela's comet, an Ephemeris of which is published in No. 1507 of the *Astronomische Nachrichten*.

I have observations of the variations of η *Argûs* extending from 1854 to the present time, which I will forward at a future opportunity. Should not this star be designated as "Variable" in the *Nautical Almanac?* It is now of the fifth magnitude.

Ring-Micrometer Observations of Encke's Comet.

1865.	Windsor M.T.	App. R.A.	App. Decl. South.
	h m s	h m s	° ′ ″
June 24	6 56 46	7 56 54·8	3 4 36
29	7 5 34	8 35 0·1	13 4 16

The first position is the result of two comparisons with B.A.C. 2660, and the second of three comparisons with B.A.C. 2975.

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Mean Places of the Stars of Comparison for the beginning of the Year, and Apparent Places for the Times of Observation.

	R.A. Mean.	R.A. App.	Decl. South. Mean.	Decl. South. App.
	h m s	s	° ′ ″	″
B.A.C. 2660	7 52 59·23	(59·93)	3 18 46·7	(60·7)
2975	8 40 0·02	(0·79)	13 3 15·3	(32·0)

Windsor, New South Wales, 17 July, 1865.

Places of Comet I. 1865 (*Great Southern Comet*), *deduced from Observations made at the Melbourne Observatory.* By R. J. Ellery, Esq.

I forward herewith the final places of the Comet I. 1865, of which I sent you a former notice.

We have only just commenced the zone work, having been much delayed in clearing up all the older observations. Next month I will endeavour to give the Society an account of the method adopted in cataloguing.

	Mean Time. Greenwich	R.A.	Log $\frac{p}{P}$	N.P.D.	Log $\frac{q}{P}$	No. of Measures.	Comparison Star.
	d h m s	h m s	+	° ′ ″	+		
Jan.	23 23 5 41·7	21 13 47·07	8·8099	133 10 12·7	9·8378	3	B.A.C. 7439
	24 22 46 54·7	21 20 19·26	8·8364	134 19 6·7	9·8047	5	Lac. 8822
	25 23 14 26·8	21 27 1·62	8·8229	135 22 40·6	9·8384	6	Lac. 8838
	26 22 53 37·9	21 33 22·92	8·8506	136 19 10·2	9·8017	5	* 7th Mag.
	27 23 29 5·4	21 39 52·92	8·8273	137 12 48·4	9·8480	4	B.A.C. 7591
	30 22 53 17·9	21 58 17·95	8·8831	139 23 52·5	9·7755	3	* 8th Mag.
Feb.	1 23 38 51·1	22 10 28·14	8·8580	140 36 7·0	9·8391	3	Lac. 9084
	2 22 55 30·8	22 16 9·47	8·9011	141 6 39·7	9·7621	5	Lac. 9111
	3 22 34 56·4	22 21 52·46	8·9176	141 35 8·0	9·7133	4	Lac. 9152
	4 22 18 3·5	22 27 32·40	8·9284	142 1 38·9	9·6666	4	B.A.C. 7862
	7 22 26 27·2	22 44 16·36	8·9388	143 10 15·3	9·6678	9	Lac. 9280
	8 22 25 49·5	22 49 43·37	8·9444	143 29 35·1	9·6600	4	ζ Gruis
	9 22 27 14·8	22 55 6·16	8·9458	143 47 35·2	9·6579	3	Lac. 9335
	12 23 30 40·4	23 10 59·64	8·9215	144 33 59·6	9·7850	4	B.A.C. 8148
	16 22 49 48·8	23 30 51·31	8·9571	145 19 38·2	9·6848	3	B.A.C. 8179
	27 23 31 19·8	0 21 9·36	8·9503	146 27 52·3	9·7569	4	* 8th Mag.
Mar.	1 23 35 3·7	0 29 38·44	8·9493	146 34 15·9	9·7626	6	* 8½ Mag.
	4 22 24 30·2	0 41 45·57	8·9813	146 41 25·6	9·5777	2	Lac. 182
	6 23 16 40·2	0 49 56·43	8·9641	146 45 5·1	9·7202	2	* 7½ Mag.
	16 22 39 30·2	1 27 48·28	8·9802	146 51 59·2	9·6275	2	B.A.C. 521
	19 22 24 57·9	1 38 32·14	8·9830	146 52 16·1	9·5864	6	Lac. 512

Until February 9th the observations were made with the spider-line micrometer; on February 10th, 13th, and 17th, two

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thick silver wires were used, one coinciding with a meridian, the other at an angle of 45° to it. For the remainder of the observations an ordinary wire-micrometer was used, the wires being of thick silver, so as to dispense with lamp illumination. All the comparison stars have been since observed with the transit circle, most of them three times at least, none less than twice. The observations have been corrected for refraction.

Melbourne Observatory, August 25, 1865.

The Astronomer Royal exhibited a Quadrant, the work of Abraham Sharp, lately received at the Royal Observatory, Greenwich, and gave the following account of it:—

The first mention of this Quadrant is in Smeaton's paper in the *Philosophical Transactions*, vol. lxxvi. for 1786, "On the Graduation of Astronomical Instruments;" a paper which, though now entirely superseded as to its fundamental methods by the process introduced by Troughton, is still in many respects worthy the attention of the astronomical-instrument-maker. Speaking of Sharp, he says (page 7), "I believe there is now remaining a quadrant, of 4 or 5 feet radius, framed of wood, but the limb covered with a brass plate; the subdivision being done by diagonals, the lines of which are as finely cut as those upon the quadrants at Greenwich."

In Hutton's *Dictionary*, article "Sharp," this statement (with many others) is copied from Smeaton.

In the *Philosophical Magazine*, vol. xxx. for 1847, January–June, page 25, is a paper by the Rev. N. S. Heineken, "On Abraham Sharp's Mechanical Productions." Numerous instruments are mentioned, and among them, this Quadrant, which Mr. Heineken was so fortunate as to secure from a tinman, who was about to use the material as old brass for the manufacture of kettles.

In the present autumn, Mr. Heineken liberally offered this relic to the Royal Observatory, with the expression of his hope that it might be preserved there with the care usually given to instruments of historical interest. The Astronomer Royal had no difficulty in making this engagement, as far as his personal control could extend; and the instrument, separated into parts as was required for the convenience of carriage, was immediately sent from Mr. Heineken's residence at Sidmouth to Greenwich. Mr. Heineken pointed out that one piece of wood was wanting; this has been supplied at the Royal Observatory, and in this state of comparative completeness the Quadrant was exhibited to the Society.

The wooden frame of the Quadrant consists of three radial bars of oak (of which two are at right angles, and the third,

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supplied at Greenwich, makes angles of 45° with the others), and two arch-pieces of oak, one of about 50° and the other of about 60°, whose ends meet in the middle of the quadrant, with mortice-and-tenon connexion, but without any double-curb structure or frame for breaking joint. Upon the three wooden radii are three iron radii and four other iron braces and connecting pieces, screwed to the wood by iron screws tapped into the wood. There is also an iron band (in two pieces, united by an intermediate piece) surrounding the wooden arch; and to this band the brass arch is attached by ten angle-irons. The brass arch itself is made of very thin sheet-brass; it is 10 inches broad in the middle and somewhat broader near the ends. It is composed of four pieces; two of the unions (near the two ends of the arch) are of dovetailed or embattled ends, brazed together (as in the old Greenwich quadrants); the central union, at 45°, does not appear to be brazed; the union seems to be effected by riveting the two parts to an iron plate below. A hole, corresponding to the centre of the graduated circular arcs, was discovered in a small piece of brass which is let into the edge of one of the radial iron bars; this hole is only about ¼ inch in diameter; it does not appear ever to have had any centre-work on which an index or telescope could turn. (A small pin is now inserted in it, for convenient lodgment of the point of a measuring beam-compass.) Upon the sheet of brass are several graduated circular arcs, one of which is partly cut away by the form given to the interior edge of the brass sheet; but the finished graduation is a band 1 inch broad very near the exterior edge of the brass sheet; its inner radius being about $4^{ft.}\,11^{in.}$, and its outer radius about $5^{ft.}\,0^{in.}$. Its graduations extend at one end about 3° beyond the quadrant and at the other end about 14° beyond the quadrant. Each degree is divided into twenty parts of 3′ each, at the exterior and interior defining curves of the band; and the graduation-points are connected by diagonal lines. Four intermediate circular arcs divide the breadth of the band into five parts; so that, with a radius properly shaped, an arc would by eye be read to 36″. The diagonal lines are very well cut, but are not (as stated by Smeaton) comparable to those on the Greenwich quadrants.

It is not easy to imagine for what purpose this Quadrant was made. Some bolt-holes in the wooden radii seem to make it probable that it has been fixed to a wall. But there is no appearance that it has ever carried an index or movable radius. The frame, throughout, is miserably weak, and it appears very strange that an experienced workman should have connected such a careful graduation with such a feeble basis, whatever were the purpose for which it was to be used.

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Adverting to the occultation-diameter of the Moon as determined in Mr. Oudeman's papers (a determination which, from his personal acquaintance with the author, the Astronomer Royal accepted as worthy of the highest credit), the Astronomer Royal remarked that it was necessary, in comparing different determinations, to keep in view the number of observations on which each was based. The Greenwich determination is founded on the comparison of 295 occultation-diameters with 440 diameters measured by the telescope under the most favourable circumstances; greater numbers probably, in both kinds, than all other observations combined can supply. He remarked that these large numbers were obtained by uniformly acting on the system of examining in each year the observations which it would be possible to make in the next year (in the instance of the telescopic semidiameter, a special calculation being necessary for every day); so that, when the time for observation arrived, every observer and every instrument were in readiness to make the observation.

The President, after tendering the thanks of the Society to the Astronomer Royal for his valuable communication mentioned that the semidiameter of the Moon, which he had been enabled to calculate from photographic observations of the Total Eclipse of 1860, coincided with the Astronomer Royal's. Those which he had selected (without knowing the Astronomer Royal's result) as best representing the observations which he had made concurred absolutely to the second decimal figure with those which the Astronomer Royal had selected as the best, namely, the occultation of a star at the dark limb, and its reappearance at the dark limb of the Moon. Mr. De La Rue, in consequence of the success of this experiment, had very great faith that astronomical photography would give hereafter, in certain classes of observations, very accurate results. In connexion with this subject he also stated that there had been a meeting the preceding day of the Lunar Committee of the British Association, and after much discussion it had been decided to make use of photographs to prepare an accurate outline map of the Moon; and it was intended to distribute portions of these maps to observers who would kindly lend their aid towards producing a more accurate representation of our satellite than existed at present. Following in the same path, he ventured to solicit amateur astronomers to send him an account of their optical means of observation, with the possibility of suggesting a distribution of work. It was proposed that the Moon should be observed in zones of about a degree in width, that every observer would have apportioned to him a zone,

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and whenever the Moon was above the horizon some part of the zone would be visible.

Those who had followed lunar photography would be aware that it had its rise in America. Perhaps he (the President) had, by his experiments and the application of time and what talent he possessed, wrested the palm from the Americans; but it was impossible always to hold the sceptre; and he was very glad to be able to say that there were two contributions, one by Mr. Draper, the son of Dr. Draper—a name celebrated in photography—and another by Mr. Rutherford, which surpassed the best which he (the President) had obtained; and he recognised this as a mark of progress in astronomical photography.

The President: I perceive on the table an Ephemeris of Biela's Comet, and I take this opportunity of stating that, having had the advantage of this and earlier ephemerides calculated by Mr. Hind, I have searched diligently, but unsuccessfully, for that comet with my 13-inch reflector.

Mr. Beck presented to the Society a set of stereoscopic views of Lunar Eclipses, by combining pictures obtained by Mr. De La Rue of the eclipses of February 1858 and October 1865.

Elements of Minor Planet (84) *Clio.*

The following set of Elements, calculated by Prof. De Gasparis from observations of August 25 and September 4, 12, and 12, are given *Astron. Nach.* No. 1554:—

Epoch, August 25·49830, Greenwich Mean Time.

Long. of Epoch	=	331° 54′ 41″·3	Mean Equinox 15 Sept. 1864.
ϖ	=	338 42 9·4	
Ω	=	327 22 28·9	
i	=	9 23 16·3	
log e	=	9·372048	
log a	=	0·373172	

The latitudes for the first and fourth observations, calculated with the foregoing elements, were

$+0° 5′ 27″·8$ and $+4° 21′ 28″·5$

the observed latitudes being

$+0° 5′ 29″·1$ and $+4° 21′ 30″·6$

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Discovery and Elements of Minor Planet (85).

The Planet was discovered by Dr. C. H. F. Peters, at the Hamilton College Observatory, Clinton, N. Y., on the 19th September, being seen as a star of the tenth magnitude. The following set of elements, calculated by Dr. Peters from observations of Sept. 20, 25, and 30, are given *Astron. Nach.* No. 1554:—

Epoch, 1865, Jan. 0, Berlin Mean Time.

			Mean Equinox 1865·0
M_0	=	329° 8′ 28″·6	
π	=	320 34 33·2	
Ω	=	204 55 5·2	
i	=	9 46 33·9	
φ	=	15 20 17·0	
μ	=	824″·104	
$\log a$	=	0·422683	

A number of copies of a paper entitled "Researches on Solar Physics," by Messrs. De La Rue, Stewart, and Loewy, have been placed at the disposal of the Fellows of the Astronomical Society. Those gentlemen desirous of receiving a copy are requested to send their names to the Assistant Secretary.

The President's *soirée* will be held at Willis's Rooms, on Wednesday, the 17th of January, 1866, at nine o'clock; for which cards of invitation are about to be issued. Gentlemen desirous of contributing objects for exhibition are requested to communicate to the Assistant-Secretary the nature of the proposed exhibits.

The attention of the Council has been called to an omission which occurs in their last Annual Report; the signatures of the Auditors ought to have been attached to the revenue account and balances for the year.

The plates containing eight representations of the planet *Mars*, to illustrate Mr. Dawes's paper, Vol. XXV., p. 225, will be issued with the next number of the *Monthly Notices*.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *December* 8, 1865. No. 2.

WARREN DE LA RUE, Esq., President, in the Chair.

Capt. Oliver Haldane Stokes, R.E., Co. Kerry, Ireland;
Henry Samuel Williams, Esq., Cambridge; and
William Jardine, Esq., 8 King's Bench Walk, Temple,

were balloted for and duly elected Fellows of the Society.

On the Determination of the Difference of Longitude between the Observatories of Greenwich and Glasgow by Galvanic Signals. By Professor Grant, F.R.S.

One of the earliest objects which I had in view after obtaining the definitive control of this Observatory in May 1860, was to take steps for effecting a new determination of the longitude of the Observatory relatively to the Royal Observatory, Greenwich, by means of some one or other of the various methods founded upon the transmission of galvanic signals, which have been used so extensively and with so much success in recent years both in the new and the old world.

But an impediment of a serious nature offered itself at the

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outset to the execution of any operation of this kind. The distance between the Observatory and the nearest telegraph station in the city of Glasgow was nearly three miles. Consequently, an electric communication could not be established with Greenwich until this hiatus was traversed by a wire, and one too which would necessarily have to pass over the roofs of the houses throughout almost the whole of its course. Fortunately, the negotiations for supplying the city of Glasgow with the advantages of correct time resulted in the Observatory obtaining the means of enabling it to overcome this difficulty. Towards the close of the year 1863 a metallic connexion was finally established between the Observatory and the office of the British and Irish Magnetic Telegraphic Company, which possesses the patent right of Jones's method of regulating clocks, and which has since been extensively employed in carrying that method into effect in Glasgow in concert with the Observatory. Shortly afterwards, having brought before the notice of the Astronomer Royal the desirableness of determining by galvanic signals the longitude of the Glasgow Observatory, Mr. Airy very kindly and liberally offered to place the resources of the Royal Observatory at our disposal, with a view to the accomplishment of this object, as soon as a convenient occasion presented itself for conducting the operations.

The next step was to obtain from one of the principal Telegraph Companies the temporary use of a wire from Glasgow to London, with the view of establishing an unbroken electric communication between the two Observatories. This was speedily effected by the good offices of Mr. F. G. Varley, the Engineer of the Electric and International Telegraph Company, who, happening to be in Glasgow at the time when the project was beginning to acquire shape, at once entered warmly into our views, and upon his representations the Company to which he is attached most obligingly assigned to us the use of one of their through wires during the whole time that it would be required for the undertaking.

The Astronomer Royal having kindly submitted to my choice several modes of conducting the operation, I selected for that purpose the method of double registration, which has been practised so successfully in the United States of America and more recently in Europe. The principle of this method is extremely simple. When a star passes each of the successive wires of the transit telescope of the more eastern of the two Observatories, the observer, by tapping a key with his finger, completes a galvanic circuit, and the instant of transit is recorded on the chronographic apparatus of the Observatory; but the galvanic current, instead of going to earth, is made to pass along the line wire to the recording apparatus of the distant observatory, upon which also the instant of transit is in the same way recorded. A process exactly similar is re-

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peated when the star comes to the meridian of the more western observatory, the instant of transit being registered on both chronographic apparatuses by the same completion of the galvanic circuit. In this manner each signalling star supplies two pairs of recorded times of transit, a comparison of the individual values of which give two distinct results, the one indicating the difference of longitude between the two observatories, and the other assigning a value of the time occupied by the galvanic current in its passage from the one observatory to the other.

The batteries and the recording apparatus used at this Observatory were courteously lent to us for the occasion by the Electric and International Telegraph Company. The insulations continued perfectly satisfactory throughout the whole period of the operations; the working of the chronographic apparatus temporarily used by us was also everything that could be desired.

In order that we might the more easily understand each other at the two Observatories, Mr. Varley made arrangements by which each Observatory was supplied with a speaking clerk from the Electric and International Telegraph Company on every night of observation.

The period of operations extended from April 28 in the present year to May 26.

The stars selected for observation amounted in number to twenty-eight, and were arranged in four groups of seven stars each, and in such a manner that when the last star of any group had passed through the telescope of the Glasgow Transit Circle, which was the more western instrument, the first star of the succeeding group was nearly about to commence its passage over the wires of the Greenwich or more eastern instrument.

The weather on the whole was not favourable for simultaneous observations at both Observatories during the period of four weeks over which the operations extended. In several instances observations were made at Greenwich which could not be responded to from Glasgow, and in one or two instances the converse of this happened. On four nights, however—May 1, May 2, May 22, and May 25—the sky was favourable for observation at both Observatories, and it is upon the results obtained on those nights that the determination of the difference of longitude contained in this paper is exclusively based.

The observers at Greenwich were Mr. Dunkin, Mr. Criswick, and Mr. Carpenter; the observer at Glasgow was Mr. Plummer. The observations of Mr. Criswick and Mr. Carpenter have been reduced to those of Mr. Dunkin by taking into account their relative personal equations, the latest determinations of which have been kindly forwarded to me by the Astronomer Royal. The observations of Mr. Plummer, whose personal equation was also determined at Greenwich, have similarly been reduced to those of Mr. Dunkin.

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The following are the results of the operations :—

May 1.

Star.	Excess of Glasgow over Greenwich Time of Transit as recorded on Greenwich Chronograph. m	s
B.A.C. 4662	17	10·73
τ Virginis		10·87
d Boötis		10·82
26 Boötis		10·81
σ Boötis		10·64
Lalande 26696		10·67
B.A.C. 4846		10·73
Lalande 26816		10·55
o Boötis		10·64
Lalande 26975		10·57
Mean Difference ...	17	10·703 ... (A)

Star.	Excess of Glasgow over Greenwich Time of Transit as recorded on Glasgow Chronograph. m	s
B.A.C. 4662	17	10·74
τ Virginis		10·75
d Boötis		10·78
26 Boötis		10·68
σ Boötis		10·68
Lalande 26696		10·57
B.A.C. 4846		10·76
Lalande 26816		10·49
o Boötis		10·58
Lalande 26975		10·55
Mean Difference ...	17	10·658 ... (A′)

Hence combining (A) and (A′), we have

	m	s
Difference of Longitude ...	17	10·680
Time of Current's Passage ...		0·023

May 2.

Star.	Excess of Glasgow over Greenwich Time of Transit as recorded on Greenwich Chronograph. m	s
τ Virginis	17	10·77
Lalande 25818		10·62
Lalande 25892		10·73
Lalande 25943		10·63

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Star.	Excess of Glasgow over Greenwich Time of Transit, as recorded on Greenwich Chronograph. m	s
d Boötis	17	10·52
26 Boötis		10·70
e Boötis		10·69
Lalande 26696		10·72
B.A.C. 4846		10·62
Lalande 26816		10·54
o Boötis		10·60
Lalande 26975		10·75
Lalande 27718		10·70
ε Serpentis		10·85
40 Serpentis		10·77
γ Serpentis		10·79
Lalande 29100		10·61
5 Herculis		10·72
45 Serpentis		10·73
Mean Difference ...	17	10·685 ... (B)

Star.	Excess of Glasgow over Greenwich Time of Transit as recorded on Glasgow Chronograph. m	s
92 Virginis	17	10·56
B.A.C. 4662		10·62
τ Virginis		10·74
Lalande 25818		10·56
Lalande 25892		10·66
Lalande 25943		10·51
d Boötis		10·50
26 Boötis		10·60
e Boötis		10·61
Lalande 26696		10·62
B.A.C. 4846		10·59
Lalande 26816		10·47
o Boötis		10·55
Lalande 26975		10·70
Lalande 27718		10·60
ε Serpentis		10·64
40 Serpentis		10·66
γ Serpentis		10·69
Lalande 29100		10·57
5 Herculis		10·66
45 Serpentis		10·73
Mean Difference ...	17	10·611 ... (B′)

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Combining (B) and (B′), we have

		m	s
Difference of Longitude	...	17	10·648
Time of Current's Passage	...		0·037

May 22.

Star.	Excess of Glasgow over Greenwich Time of Transit as recorded on Greenwich Chronograph. m	s
σ Bootis	17	10·55
Lalande 26975		10·49
Lalande 27718		10·42
3 Serpentis		10·58
Lalande 27885		10·58
6 Serpentis		10·50
8 Serpentis		10·42
τ Serpentis		10·50
10 Serpentis		10·35
ε Serpentis		10·35
γ Serpentis		10·46
5 Herculis		10·52
45 Serpentis		10·36
Mean Difference ...	17	10·468 ... (C)

Star.	Excess of Glasgow over Greenwich Time of Transit, as recorded on Glasgow Chronograph. m	s
σ Boötis	17	10·44
Lalande 26975		10·40
Lalande 27718		10·42
3 Serpentis		10·56
Lalande 27885		10·49
6 Serpentis		10·42
8 Serpentis		10·33
τ Serpentis		10·44
10 Serpentis		10·27
ε Serpentis		10·30
γ Serpentis		10·47
5 Herculis		10·36
45 Serpentis		10·27
Mean Difference ...	17	10·398 ... (C′)

Combining (C) and (C′), we have

		m	s
Difference of Longitude	...	17	10·433
Time of Current's Passage	...		0·035

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May 25.

Star.	Excess of Glasgow over Greenwich Time of Transit, as recorded on Greenwich Chronograph. m	s
26 Boötis	17	10·52
σ Boötis		10·31
Lalande 26696		10·41
B.A.C. 4846		10·48
Lalande 26816		10·55
ε Boötis		10·48
Lalande 26975		10·38
Lalande 27718		10·61
3 Serpentis		10·49
Lalande 27885		10·51
6 Serpentis		10·42
8 Serpentis		10·33
τ Serpentis		10·57
10 Serpentis		10·48
40 Serpentis		10·48
γ Serpentis		10·48
5 Herculis		10·76
Lalande 29261		10·54
45 Serpentis		10·32
Mean Difference ...	17	10·480 ... (D)

Star.	Excess of Glasgow over Greenwich Time of Transit, as recorded on Glasgow Chronograph. m	s
26 Boötis	17	10·47
σ Boötis		10·35
Lalande 26696		10·36
B.A.C. 4846		10·41
Lalande 26816		10·52
ε Boötis		10·43
Lalande 26975		10·37
Lalande 27718		10·57
3 Serpentis		10·48
Lalande 27885		10·48
6 Serpentis		10·40
8 Serpentis		10·31
τ Serpentis		10·52
10 Serpentis		10·50
40 Serpentis		10·50
γ Serpentis		10·50

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Star.	Excess of Glasgow over Greenwich Time of Transit, as recorded on Glasgow Chronograph.
	m s
5 Herculis	17 10·61
Lalande 29261	10·43
45 Serpentis	10·24
Mean Difference ...	17 10·445 ... (D′)

Combining (D) and (D′), we have

	m s
Difference of Longitude ...	17 10·463
Time of Current's Passage ...	0·018

Collecting together the mean values of the difference of longitude for the four days, we have

	Difference of Longitude. m s
May 1	17 10·680
2	10·648
22	10·433
25	10·463

Combining these together with a due regard to the number of observations on each day, we obtain for the definitive value of the longitude of the Transit Circle of the Glasgow Observatory,

$17^m\ 10^s{\cdot}55$ W.

Similarly, for the time of the current's passage,

	s
May 1	0·023
2	0·037
22	0·035
25	0·018

Whence we obtain definitively

Time of Current's Passage = $0^s{\cdot}029$

I cannot omit this occasion of expressing my warmest acknowledgments to the local Officials of the Electric and International Telegraph Company, who, from Mr. Evans, the Superintendent, downwards, were most courteous and obliging, and rendered us very efficient aid in the course of our operations.

The Observatory, Glasgow, Nov. 9, 1865.

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On the Telescopic Disks of Stars. By E. J. Stone, Esq.

This subject having lately excited some attention amongst the Fellows of this Society, it may be worth while to recall what theory really says upon the point.

Theory informs us that if a pencil of homogeneous light diverge from one point, and after refraction through a lens, with circular aperture, converge accurately to a focus, then, instead of a single point of light at the focus, we have a small disk of light, the intensity of the illumination of which rapidly degrades to a ring of absolute blackness, then increases to a ring of maximum illumination, then decreases to a ring of absolute blackness, and so on for many alternations; the illumination, however, at the different maxima degrades with such rapidity that at the first, second, and third maximum, we have respectively the $\frac{1}{57}$th, $\frac{1}{240}$th, and $\frac{1}{620}$th of the illumination at the centre of the disk. (See Professor Airy's Paper, *Camb. Trans.* vol. v.) If we call these rings of absolute blackness and maximum intensity dark and bright rings respectively of the first, second, &c. order, theory proves that the angular radii, at the centre of the lens, of a ring of any order, is independent of the focal length of the lens, and varies inversely as the radius of the aperture. The theoretical law of the degradation of the brightness of the central disk is exhibited in the following table, which is calculated with 0·000022 inch for the wave length. The intensity at the centre of the disk is taken as the unit, and the angular distance between the centre and the first black ring is divided into twenty equal parts, for each of which the intensity is given.

Table.

	Illumination.		Illumination.
1	1·0000	11	0·3326
2	0·9903	12	0·2554
3	0·9606	13	0·1879
4	0·9134	14	0·1312
5	0·8503	15	0·0857
6	0·7746	16	0·0511
7	0·6897	17	0·0267
8	0·5994	18	0·0111
9	0·5075	19	0·0028
10	0·4175	20	0·0000

An inspection of this table shows that for some considerable distance from the black ring the intensity of the light is exceedingly feeble as compared with that at the centre. Consequently, it by no means necessarily follows from the theory that the angular diameter of the visible disk varies

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inversely as the radius of the aperture. It would be a consequence of the theory, if the visible disk always extended sensibly to the first dark ring, but this is certainly not true, for if so, the apparent disk of a faint star would be as great, under the same circumstances of vision, as that of a bright star. The visible disk would also vary inversely as the radius of the aperture, provided that, under the circumstances of vision, the theoretical disk, which extends to the first black ring, should be always proportionably contracted.

When an image of a star is formed by an object-glass well corrected for spherical and chromatic aberration, the conditions required by the theory are satisfied, except that the presence of rays of different wave lengths complicates the phenomena by introducing coloured rings which will not be considered here. It would appear, then, that, if we should attempt to compare the measures of star-disks made with telescopes of different apertures, theory would not necessarily require these measures to be in the exact inverse ratios of the radii of the apertures. It would, however, require that any diminution of brightness in the image, any increase of magnifying power applied to the telescope, should be followed by a tendency in the measures to come out smaller.

The well-known contraction of the images of a star, when haze passes over the star, is one illustration of a diminution of brightness followed by a decrease in the apparent size of the disk; and with respect to the effects of increasing the magnifying power, which comes to the same thing, we cannot do better than quote some remarks by Mr. Dawes, in the *Astr. Nach.* No. 1552. "The telescopic star-disks do not increase in proportion to the power employed on the same aperture; and, consequently, up to a certain limit, the separation of disks (in his crystal micrometer) increases with the magnifying power."

At the June Meeting of this Society, Mr. Dawes stated "that he had often measured the disks of stars with a double-image micrometer formed of double refracting crystal, and that, under *similar circumstances*, he found the measured disks to depend wholly and entirely on the aperture of the telescope."

This statement of Mr. Dawes was claimed as a verification of the undulatory theory.

I think enough has been said to prove the extreme delicacy of any attempt to verify the theory by the measurement of star-disks. The inquiry is here complicated by our ignorance of what theoretical illumination is under given circumstances required to constitute the visible boundary of the image.

It would, at least, be necessary to know more exactly the force of, "under similar circumstances," before claiming Mr. Dawes' statement as a verification of theory. We should require to know whether all the observations were made with the same crystal micrometer, and whether the same eye-piece

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was always used, or if the eye-pieces were changed so as to secure the same magnifying power on the different telescopes, if the law was found to hold true over any considerable range of powers. If Mr. Dawes has any accurate observations on the apparent magnitudes of star-disks, their publication would be a great boon to all interested in the subject of physical optics.

1865, *Nov.* 7.

In the discussion on this paper,

Mr. Pritchard asked whether Mr. Stone considered that the diameter of the spurious disk in some degree depends upon the perfection of the object-glass; and whether, if the spherical aberration in one telescope were better corrected than in another, it would, *cæteris paribus*, have a tendency to reduce the size.

Mr. Stone replied in the affirmative.

Mr. Pritchard observed that he had been induced to ask the question because Mr. Dawes had stated that the size depended, *cæteris paribus*, entirely upon the aperture, which was quite in accordance with theory. A conversation took place between himself and Professor W. H. Miller, of Cambridge, wherein reference was made to some object-glasses made by Dr. Steinheil, who, according to Professor Miller, had stated that he had made object-glasses of such perfect form and material, that the spurious disk of the stars became so large that for double-stars, which appeared to be divided when viewed through ordinary telescopes, the spurious disks were so expanded that a dark glass was necessary to see whether they were divided or not!

The speaker added that the spurious disk of a star diminishes very rapidly in brightness towards the edge. No doubt the more diaphanous the material, the less light will be absorbed, and the larger the spurious disk will (so far) appear; but the glass must be diaphanous beyond all precedent for any one to be able to measure the difference in the size of the disk. In fact, measurement can scarcely apply to the spurious disk. It depends partly upon the light of the sky; and any illumination in the telescope would diminish it. Mr. Pritchard thought that, for the purposes of measurement, we must look to the diameter of the first ring, and he would ask whether Mr. Stone had measured the diameter of the brightest part of that ring; and if so, whether it did or did not vary inversely with the aperture. If it did not, then the undulatory theory must be re-examined, as there was a residual phenomenon from which something was yet to be learned. As far as the effects of spherical aberration are concerned, he (Mr. Pritchard) was

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perfectly sure that there never was a tolerable telescope made in modern times which had the slightest influence upon the diameter of the spurious disk. A telescope possessing an amount of spherical aberration that would interfere with the diameter of the disk would be such a telescope as no honest man would venture to sell, and no skilful man trouble himself to use. The diameter of the brightest part of the first ring he looked upon as an *experimentum crucis* of that part of the undulatory theory; he was, therefore, desirous of hearing Mr. Stone's opinion upon the subject.

Mr. Stone remarked, in reply, that he had never made any such measurement. At the last Meeting some references which he believed to be quite illusory were made to the question, and the only object he had in writing his paper was to point this out. The undulatory theory only referred to the rings of absolute blackness and maximum illumination, and with regard to those no doubt it was true; but as to verifying the theory by mere measures of the disk, he believed it would be fallacious.

On Personal Equation in Reading Microscopes.

By E. J. Stone, Esq.

In the *Monthly Notices* of the Royal Astronomical Society 1865, May 12, is an abstract of an interesting paper by Mr. Dunkin, "On some Peculiar Instances of Personal Equation." One of these illustrations is obtained as follows: Mr. Dunkin has selected from the "Greenwich Results" the observations of *Polaris* and *Polaris S.P.*, made by Messrs. Dunkin, Ellis, Criswick, Lynn, and Carpenter, all experienced observers; from the discrepancies of the results of the observations made by each observer above and below the pole, Mr. Dunkin has deduced corrections to the adopted colatitude, and has considered each observer to have his own colatitude.

The results thus obtained are as follows:—

From Mr. Dunkin's observations	...	...	38°	31′ 21″·81
,, Mr. Ellis's	,,	...	38	31 22·01
,, Mr. Criswick's	,,	...	38	31 21·92
,, Mr. Lynn's	,,	...	38	31 21·64
,, Mr. Carpenter's	,,	...	38	31 21·48

The difference between the results thus obtained from the observations of Messrs. Ellis and Carpenter is seen to amount to 0″·53.

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On the night this paper was read, I offered to the Meeting what I considered the explanation of these anomalous results, an explanation, however, perfectly in accordance with the general result of Mr. Dunkin's paper "that personality does exist in the reading of microscopes," and, in fact, a necessary consequence of the existence of such personality; and finding that explanation borne out by a numerical investigation, I have thought it worth while to place it on record.

The stability of the Greenwich Transit Circle is such that the reading corresponding to the zenithal position of the telescope remains sensibly unchanged for considerable periods of time; it has been usual to put together all the determinations of zenith-point reading made in a fortnight, and to adopt the mean result for the zenith-point reading in the reduction of all observations made during that fortnight.

The observing with the Transit Circle is distributed with great regularity amongst the assistants. If therefore, we call the differences of habit in reading the microscopes between any one observer and the mean habit of all the observers the personal equation of that observer, it will follow from the course adopted at Greenwich that the adopted readings for the zenithal position of the telescope will be free from personal equation, but that the resulting observations in N.P.D. will be affected with the personal equation of the observer, consequently the difference between the reading corresponding to the zenithal position of the telescope and the reading corresponding to the polar position of the telescope, as determined from the observations of any one observer, will be affected with the personal equation of the observer in opposite sign.

If this explanation is correct, the mean of all the results, which is the colatitude adopted at Greenwich, is free from the error of personality, and of course if the same observer determines the zenithal position and polar position of the telescope, the colatitude is free from personal equation; this is on the assumption that the observer's personal habit does not sensibly change in reading off different divisions of the circle.

In order to test this explanation numerically, I have found the differences between the nadir-point determinations made by Messrs. Dunkin, Ellis, Criswick, and Carpenter; to do this I have compared all the derminations made by these gentlemen during the year 1861 and 1862. I find

Mr. Dunkin's Reading for Nadir	— Mr. Ellis's ditto	$= +0''.30$
,, ,,	— Mr. Criswick's ditto	$= +0''.04$
,, ,,	— Mr. Carpenter's ditto	$= -0''.35$

There were not a sufficient number of Mr. Lynn's observations to determine his personal equation.

We thus have for the personal equations,

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D. = 0·00
E. = + 0·30
C. = + 0·04
J. C. = − 0·36

And applying these results, with changed signs, to the colatitudes deduced by Mr. Dunkin, we have for the resulting colatitudes,

From Mr. Dunkin's observations	...	...	38° 31′ 21″·81
,, Mr. Ellis's	,,	...	38 31 21·71
,, Mr. Criswick's	,,	...	38 31 21·88
,, Mr. Carpenter's	,,	...	38 31 21·83

The agreement between these results is very close indeed. It appears that Mr. Ellis's and Mr. Carpenter's habits of reading microscopes, although very different, do not sensibly vary with the part of the circle used. It may be mentioned that Mr. Carpenter is longer sighted than Mr. Lynn, Mr. Lynn than Messrs. Dunkin and Criswick, and Messrs. Dunkin and Criswick than Mr. Ellis. It is impossible, on account of changes of temperature, to keep the graduations of the circle and the microscope-wires always at the same time perfectly in focus. I am inclined to think that the amount of personality in reading the microscopes would be much diminished, if the wires and graduations were always seen under exactly similar optical circumstances.

Upon the question of Personal Equation some discussion took place.

Mr. Pritchard mentioned the case of a great practical philosopher whom he had known, who was not able to bisect twice running within three seconds, and he doubted whether anybody in the room not habituated to observe, would be able to avoid a similar mistake. On the other hand, he had tested the eyes of the late Mr. Andrew Ross, the microscope-maker, some years ago. Mr. Ross was not accustomed to astronomical observations, and yet he bisected a division of a circle ten times, without making a difference of a 10th of a second.

Professor Challis mentioned an instance of an assistant at the Cambridge Observatory, who was a very good and consistent observer, but in bisecting with a microscope his results were always erroneous, about $\frac{3}{10}$ or $\frac{4}{10}$ of a second.

Mr. Dunkin gave another noteworthy instance, which came under his eye only a few days previously; and at the end of the discussion which took place upon the subject, Mr. De La Rue concluded by calling attention to the importance

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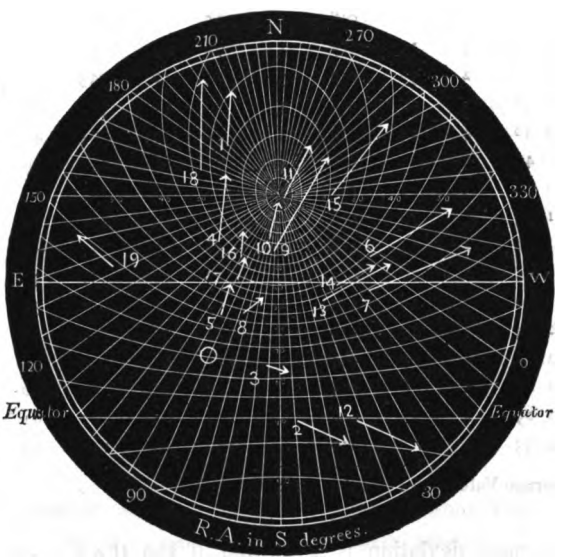
of making such comparisons so as to eliminate personal error. We were too apt to look upon graduated instruments as leading to almost absolute perfection in observation. But we must always bear in mind that the eye forms part of the instrument and must take that into consideration.*

Radiant Points of Shooting-Stars. By A. S. Herschel, Esq.

The following remarks relate to a meteoric shower of some intensity, and displaying a remarkably radiant definite point, observed at Hawkhurst on the 20th of October, 1865.

On the nights of the 16th–18th of October, the sky at Hawkhurst was overcast, with constant rain. On the night of the 19th, a watch was kept for forty-five minutes, until eleven o'clock, but, although the sky was clear, without seeing any meteors directed from the known radiant point in *Orion*. On the night of the 20th the sky was cloudless, and on the watch being resumed, the



* An artist of celebrity expresses a doubt regarding the existence of personal equation in the estimation of the bisection of a division of a circle. A reading microscope, free from parallax, is in process of construction; great attention will be paid to the cleanness of the cut, to the illumination, and to the regulation of the emergent pencil; and then the Members of the Society will be able to make the experiment themselves at an evening meeting.—[C. P.]

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occurrence of this meteoric shower was immediately apparent. Owing to their leaving remarkably luminous streaks, the tracks of nineteen meteors, described in the following list, could be noted among the stars with somewhat more than ordinary precision. Projected upon a plane-perspective planisphere of the sky (see figure), having the zenith of Greenwich for the centre of the projection, these nineteen tracks, with three exceptions, when prolonged backwards, pass through a small circle having a radius of only four degrees, and having its centre situated at *ν Orionis*, in R.A. 90°, N. Decl. 15°. In the following table the deviation of each meteor from the general radiant point; or the least distance of the great circle of its apparent path, prolonged backwards, from this point, is given in the last column of the table:—

No.	G.M.T. 1865. Oct. 20.	Mag. Compd with Stars.	Length of Path.	Duration.	Streak Duration.	Began. R.A.	Began. N. Decl.	Ended. R.A.	Ended. N. Decl.	Deviation.
	h m		°	sec.	sec.	°	°	°	°	°
1	10 30	3	9	0·4	...	189	69	210	65	2·8
2	10 43	3	14	0·7	...	53	0	40	−5	3·0
3	10 58	2	9	0·8	1	67	14	58	12	2·8
4	11 6	3	21	0·8	1	125	61	171	70	0·2
5	11 19	3	15	0·7	2	92	29	95	45	0·2
6	11 22	2	39	0·9	3	15	46	333	37	2·0
7	11 31	2	30	0·8	3	16	35	342	28	0·1
8	11 36	4	13	0·6	1	81	32	73	44	0·4
9	11 40	3	41	0·9	2	70	66	297	70	0·6
10	11 41	3	22	0·7	2	84	68	17	89	3·0
11	11 43	3	18	0·6	1	17	89	276	73	4·0
12	11 48	2	24	0·8	2	38	0	17	−7	0·2
13	11 54	2	26	0·7	2	33	39	357	37	0·0
14	12 0	3	16	0·5	1	23	40	2	38	0·0
15	12 4	3	17	0·6	1	322	70	303	56	9·5
16	12 18	4	9	0·4	1	101	59	109	68	1·8
17	12 25	5	6	0·4	...	95	45	99	60	1·2
18	12 29	2	17	0·6	2	164	62	201	61	5·0
19	12 33	2	8	1·0	...	130	29	141	29	6·0
	Average Values		19	0·68	1·3					2·2

The mean deviation is 2°·2: but if the three most perturbed of these nineteen meteoric tracks (Nos. 15, 18, and 19) are omitted, the mean deviation from the general radiant point, of the sixteen remaining meteors is only 1°·4. The deviations of fourteen meteors from their common radiant point, observed on the 18th of October, 1864 (*vide Monthly Notices* for 1864, December 9), were in the same manner as follows:—

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Ref. No.	Deviation.	Ref. No.	Deviation.	Ref. No.	Deviation.	Ref. No.	Deviation.
	°		°		°		°
1,	2·8	5,	3·0	9,	1·8	13,	1·5
2,	2·5	6,	0·4	10,	6 0	14,	0·2
3,	0·3	7,	1·7	11,	10·6	Mean Deviation	2·9
4,	0·0	8,	3·0	12,	7·2		

If the three most perturbed of these fourteen meteoric tracks (Nos. 10, 11, and 12) are omitted, the mean deviation from the common radiant point of the eleven remaining meteors is only 1°·6. Collecting together the results of both years' observations, we are thus enabled to give the position of this radiant point in the following definite, tabular form: —

Radiant O. Date of Observation.	Position. R.A.	Position. N. Decl.	No. of Meteors Observed.	Mean Deviation.
	°	°		°
1864, October 18	90	16	11	1·6
1865, October 20	90	15	16	1·4

As the meteors of this shower will probably be observed every year, it will be interesting to ascertain if the radiant point so distinctly marked, and apparently so fixed, will in future undergo any alteration in its position. A few bright meteors were observed at Newhaven, U.S., by Herrick, on the nights of the 20–26th of October, 1839 (vide *Am. Jour. Sci.*), and were described by him as radiating from "near ι *Geminorum*." This position is more than twelve degrees from the place assigned to the present radiant point. It is not impossible that the radiant point of a meteoric shower may have altered its position to this extent in the course of nearly thirty years. Connected with the question of the possible motion of the radiant point, every available means will be taken by the Luminous Meteor Committee of the British Association, to determine as accurately as possible the position of the radiant point of the November meteors, on the occasion of their expected return on the morning of the 13th of November, 1865.

The November Meteoric Shower. By J. Glaisher, Esq. F.R.S.

[This was a verbal communication to the Meeting.]

At the last Meeting of this Society, notice was given by the President that a recurrence of the November shower of meteors was expected on the 12th of November. I believe that notice was responded to by a very large number of observers, who looked out very assiduously till the hour of 12, and then retired without occupying themselves at all about

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meteors afterwards. I am not surprised at this. It is the custom of the Astronomer Royal to enter in a pocket-book, before the end of one year, all phenomena to which attention is to be paid in the following year; and if we look into these pocket-books, we shall find in every one of them, as far back as 1836, entries on certain days, "Look out for meteors;" and if we look into the Greenwich volume of *Observations* for the observations of meteors on those days, we shall find very few recorded; yet on every one of these days I have charged observers to keep a good watch on certain parts of the heavens, until at last I was almost ashamed to do so, and almost concluded that the shower-meteor theory could not be sustained.

On the night common to the 12th and 13th of November a watch was kept up at the Royal Observatory, Greenwich, from the hour of 6. Between this hour and 8^h two meteors only were seen; from 8^h to 12^h the sky was cloudy and no meteors were seen. At this time six observers (viz., W. C. Nash, A. Harding, F. Trapaud, E. Jones, T. Wright, and Lieut. Rikatcheff, R. I. N.) were located on an elevated part of the observatory. At 12 minutes after midnight the clouds began to break, and at 20 minutes to 1 the sky was free from clouds. At 1 o'clock the paths, zones, colour, and other particulars of 29 meteors, were recorded. By 2^h A.M. the same particulars of 70 additional were noted; and by 5^h A.M. the positions, &c. of nearly 280 meteors had been secured. Before this time we knew that we had abundance of observations to determine the radiant point, or points; and for a space of nearly a quarter of an hour the paths of the meteors among the stars, &c. were not noticed, but their number was simply counted. The result was that at this time meteors of the first class were appearing at the rate of 250 per hour. Now for every meteor observed, there were at the lowest estimation two or three whose positions were not recorded; so that at least 1000 meteors were visible during the hours of 1 to 5 o'clock.

At the hour of 5 the Moon was shining brightly, and many meteors were seen close to her.

Mr. Alexander Herschel was observing at Hawkhurst till nearly 3^h A.M., and he noted the positions and paths of 68 meteors, which he has laid down on a diagram, indicating very clearly a well-marked, radiant point in *Leo*.

Many of the Greenwich meteors are laid down on this diagram, and by comparing the two diagrams together it will be seen that the Hawkhurst diagram indicates the radiant point in *Leo*, within narrower limits than the Greenwich diagram. Also it will be seen that more than one radiant point is indicated in the Greenwich diagram; but they are in the same right ascension, viz., about 10^h. Now the great interest connected with this part of the results, in relation to astronomy, is that this position in *Leo* is the part of the heavens towards which the Earth was moving at this time; and apparently, as

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soon as the Earth approaches this part of the heavens, those bodies situated there become luminous. This adds another link to the sciences of Astronomy and Meteorology.

I think there is no doubt that the meteors observed this night were those of the November period. At Greenwich, for more than twenty years, a good look-out has been kept for the annual display of shooting-stars at the November period, but this is the first time we have seen them. It is very likely that on several occasions we have closed our watch too early. It was the knowledge of this fact that made me say, at the beginning of these remarks, that I was not surprised at gentlemen giving up watching at 12 o'clock; for it is no joke, as I know by experience, to stand motionless on a clear, cold night, hour after hour, staring, as it were, at vacancy, with perhaps one or two observations only as a reward. It is also likely that on some occasions the shower has taken place during the day, and therefore has not been visible at Greenwich at all.

Speaking of radiant points, I may remark that there are 56 already determined and well marked. Many of these determinations have been materially influenced by isolated observations of meteors observed on the same day in different years being brought together, thus indicating the same point of the heavens as the origin of the meteors. I know nothing in physical research which seemed so unpromising as isolated observations of shooting-stars did a few years ago, yet how well a patient and painstaking record of such phenomena has been repaid.

The next step in the reduction of the observations of these meteors will be to ascertain those which have been doubly and trebly observed, so to determine their heights and velocities.

The only information I can give to-night in these respects is that with which Mr. A. Herschel has furnished me. It appears that 15 meteors observed on that night by Mr. Herschel at Hawkhurst were simultaneously observed by Professor Adams at the Cambridge Observatory. The distances from the Earth, at the beginning and end of the apparitions of five of these doubly-observed meteors, were as follows:—75 miles and 54 miles; 72 miles and 55 miles; 68 miles and 44 miles; 89 miles and 57 miles; and 114 miles and 86 miles.

These distances are somewhat greater than usual. I consider it of great importance to get simultaneous observations of other meteors; and I hope that our knowledge of the distances &c. of these bodies will be much increased by the observations of this night.

Mr. Pritchard: There is one matter I would ask Mr. Glaisher to state to the Meeting; of course the number visible of these shooting-stars depends upon the radiant point being above the horizon?

Mr. Glaisher: Yes.

Mr. Pritchard: And therefore these observations prove

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how correctly the position of this radiant point is known, because, from what I gather from your communication, it appears that the maximum number of meteors occurred just at the hour that was foretold.

Mr. Glaisher: It was within one hour or so. The hour foretold was from 12 to 1 A.M.; two-thirds of that hour was cloudy. I think the President, at the last Meeting, recommended looking out for an hour or two before and after the hour of 12. I may mention that a fine display is expected on the 13th of November next year.

Mr. Pritchard: Did you see the maximum number of meteors about the time when the point of the heavens towards which the Earth was moving appeared above the horizon?

Mr. Glaisher: Most certainly; and I may add that in the six hours preceding midnight on the 12th, we saw only two meteors, and in the six hours preceding midnight on the 13th, with a cloudless sky, two meteors only were seen; thus it would seem that both before and after this time the meteors were scarce.

Mr. Pritchard: What time did that part rise?

Mr. Glaisher: Before midnight; it would be about six hours from the meridian at midnight and about two hours from the meridian at 5 in the morning.

Mr. Pritchard: And your maximum was seen somewhat after midnight certainly.

Prof. Challis: What Mr. Glaisher has said confirms my experience at the Cambridge Observatory. I often looked out for the November meteors, but could not see them. I had seen the August meteors very regularly with no very great disproportion as to the numbers; but I cannot say that I saw anything more on the 12th November than on other nights. Professor Adams has given me the particulars of his observations last 12th November. He begun at 12 o'clock, and went on to about 20 minutes past 1, and during that time I think it was 120 — at all events, above 100, that he saw. My experience of the August meteors has been this, that I never at that hour of the night got that number; so that it is quite clear, from the experience of the Cambridge Observatory, that the appearance of this last November is exceptional — a kind of shower such as we have not noticed before. I may also, perhaps, state that Professor Adams told me that he found fourteen coincidences — I think twelve certain — by comparing with Mr. A. Herschel's observations: and that the average height of the meteors they ascertained was 83 miles. I mention this because, in the year 1862, I took observations myself in the August period, and compared them with observations made at Hawkhurst on that occasion, and we got an average height of 82 miles. The number of coincidences is ten; and I think the coincidence of heights at the two periods is remarkable and worthy of notice.

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Mr. Prideaux said that on the night in question he had gone with two companions to the top of Primrose Hill, and that in the forty minutes they remained there they saw seventy-two meteors. His friends were perfectly inexperienced in the matter. They arrived there about 3 o'clock in the morning. During the whole of the time that they were out, somewhere about an hour and a half, they saw nearly 120 meteors.

Observations of Occultations of Stars by the Moon, and Phenomena of Jupiter's Satellites, made at Mr. Barclay's Observatory, Leyton, N.E. By C. G. Talmage, Esq.

1865, June 28. Transit of *Jupiter's* First Satellite.

Egress.

		h m s	
G.M.T.	Central bisection =	10 31 3	*a*
	Last contact =	10 33 23	

1865, July 5. Disappearance of 8 *Libræ*.

G.M.T. = $9^h\,44^m\,45^s{\cdot}45$ *b*

Disappearance of α^2 *Libræ*.

	h m s		
G.M.T.	9 58 4·24	Mr. Barclay	*c*
	9 58 6·24	Mr. Talmage	*d*

1865, July 3. Reappearance of 8 *Libræ*.

G.M.T. $10^h\,48^m\,29^s{\cdot}46$ *e*

Reappearance of α^2 *Libræ*.

G.M.T. $10^h\,51^m\,22^s{\cdot}49$ *f*

1865, July 8. Occultation of ζ' *Sagittarii*.

		h m s	
G.M.T.	Disappearance =	9 21 19·13	*g*
	Reappearance =	10 11 30·88	*h*

1865, July 25. Eclipse: Reappearance of *Jupiter's* Second Satellite.

G.M.T. $10^h\,41^m\,0^s{\cdot}60$ *k*

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1865, July 26. Eclipse: Reappearance of *Jupiter's* Third Satellite.

G.M.T. $8^h\,44^m\,32^s{\cdot}08$ *l*

1865, July 29. Eclipse: Reappearance of *Jupiter's* First Satellite.

G.M.T. $10\,17^m\,19^s{\cdot}43$ *m*

1865, Nov. 4. Occultation of δ^2 *Tauri.*

		h	m	s	
G.M.T.	Disappearance	9	57	56·93	*n*
	Reappearance	10	36	43·06	*o*

a. ***Jupiter*** low, but the satellite was well defined.

b. Observed with the large refractor of 10 inches aperture, with a power of 137; time exact.

c and *d.* Mr. Barclay observed the disappearance of this star with the large telescope; but he thinks the time a little doubtful. I observed it with a small telescope of $3\frac{1}{4}$ inches aperture, placed in the garden; and I feel confident in the time being exact.

e and *f.* Both stars came out very sharp at the times; the night was very clear. The colours of the stars contrasted very strongly, α^2 *Libræ* being almost white and 8 *Libræ* grey.

g and *h.* The star did not disappear instantaneously, but hung on the Moon's limb for nearly half a second, or rather it seemed to be elongated considerably. The time of reappearance is exact.

k. Observed with a power of 220 on the equatoreal; very clear and steady.

l, *m*, *n*, and *o*. The times are exact.

The eclipse of the Moon, on the 4th of October, was well seen here, but no attempts were made to take time observations; the dark limb of the Moon was apparently surrounded by a luminous streak, as noticed by Mr. De La Rue; but it did not disappear on applying higher powers; the highest used was 350, on the 10-inch refractor; this was remarked by Mr. Barclay as well as by myself.

Observations of the Comet III. 1860, *made at the Santiago Observatory, Chili.* By C. W. Moesta, Director of the Observatory.

I beg leave to transmit a series of observations of the Comet III. 1860, some of which were, I believe, sent in the year 1861. Those observations were made with a 5-feet Equatoreal, to which was adapted a ring-micrometer; but as

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at that time the new building of the Observatory was not finished, the instrument could not be mounted equatoreally, and could only be placed provisionally on loose ground in a room, provided with a prime vertical opening. On account of this circumstance, I consider the enclosed observations of no great value; still, if the Comet should not have been observed at that time elsewhere in a better way, it might be deemed proper to publish them.

1860.	Santiago Mean Time. h m s	Δα. Comet—Star. m s	No. of Comps.	Δδ. Comet—Star. ′ ″	No. of Comps.	Comp Star.
July 21	7 10 49·3	+ 5 39·17	2	...	...	1
24	6 35 27·9	+ 3 49·18	3	...	...	2
25	6 53 59·2	...	...	+44 18·7	2	4
Aug. 13	9 50 27·6	+ 6 16·29	2	+40 18·5	2	5
	10 10 25·9	− 10 4·87	2	...	...	6
17	9 23 59·0	− 5 25·56	2	...	...	7
	10 4 32·4	...	...	+ 47·6	2	7
	10 10 21·2	...	...	+37 35·7	1	8
18	9 17 10·8	+ 4 42·70	3	...	...	8
	9 45 36·4	...	...	+41 43·8	3	9
20	9 6 53·2	+ 3 58·87	3	...	...	10
	9 31 57·2	...	...	− 1 43·1	2	10+
	9 50 57·1	...	...	− 50·5	2	10−
23	8 54 28·6	+ 3·11	3	...	...	11
	8 54 28·6	− 2 59·39	3	...	...	12
	9 35 12·2	− 2 55·19	2	...	...	12
	9 21 3·7	...	...	+ 7 14·3	...	12
Sept. 6	7 58 34·7	− 2 51·29	3	...	...	13
	8 39 13·1	...	...	− 51·3	2	13+
	8 57 14·9	...	...	− 12·4	2	13−
7	8 4 27·2	− 40·88	1	...	...	13
8	8 32 36·3	+ 1 37·00	1	...	...	
10	9 10 21·5	...	...	+39 4·8	2	17
11	7 44 4·6	− 6 34·31	2	...	...	18
	8 16 45·7	...	...	+ 6 8·2	1	18
	8 33 19·7	...	...	+ 1 58·9	1	19+
	8 45 8·8	...	...	+ 2 44·8	2	19−
12	8 8 43·8	...	...	+ 8 1·8	2	20+
	8 27 16·8	...	...	+ 7 59·1	2	20−
13	7 26 23·8	− 1 16·64	2	...	...	21
	7 45 46·1	...	...	+ 18·0	2	21+
	8 4 29·3	...	...	+ 1 17·2	2	21−

The signs + and − in the last column indicate that the comet and the star were observed on both sides of the centre of the micrometer.

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Mean Places for 1861·0 *of the Comparison Stars,*

Deduced from the Observations made with the Meridian Circle at Santiago.

No.	Star's Name.	Mag.	R.A.	Decl.	Prec. In R.A.	Prec. In Decl.
			h m s	° ′ ″		
1	Arg. O. 11830	6½	11 53 44·93	−18 53 7·2	3·060	20·05
2	Lacaille 5125	7·6	12 15 46·41	−24 6 6·0	3·031	20·01
4	B.A.C. 4253	5½	12 30 20·37	−26 22 12·6	3·160	19·88
5	ν Centauri	4	13 41 11·12	−40 59 36·0	3·569	18·13
6	λ Centauri	3½	13 57 34·71	−40 30 41·5	3·633	17·48
7	Anon.	7	14 4 52·48	−41 48 34·2	3·690	17·15
8	Lacaille 5809	7	13 57 37·04	−42 25 24·5	3·672	17·47
9	Lacaille 5826	6	14 0 15·32	−42 48 27·9	3·692	17·36
10	Anon.	8	14 3 54·82	−42 42 11·6	3·707	17·19
11	Lacaille 5918	7	14 15 51·40	−43 40 34·4	3·785	16·63
12	Lacaille 5943	7	14 18 54·12	−43 42 6·5	3·80	16·48
13	Anon.	7½	14 52 28·91	−46 27 2·7	4·033	14·64
17	Anon.	7	14 57 52·83	−47 45 26·3	4·103	14·31
18	Lacaille 6279	6	15 7 14·59	−47 20 46·1	4·13	13·73
19	Anon.	10	14 58 43	−47 17		
20	Taylor 8002	7	15 6 38·86	−47 31	4·134	
21	Taylor 7997	...	15 6 15·03	−47 33 11·1	4·133	13·79

June 2, 1865.

Observations of Solar Eclipse October 19, 1865.

By the Rev. Temple Chevallier.

I send the observed time of the first contact of the Solar Eclipse of October 19, 1865, made by Mr. M. R. Dolman, observer, for insertion in the *Monthly Notices*.

Solar Eclipse, October 19, 1865.
Observation of Time of First Contact, made at the Observatory, Durham.

1865, Oct. 19, $4^h\ 7^m\ 34^s{\cdot}7$ Greenwich Mean Time.

Good Observation.

M. R. Dolman.

Durham Observatory, Nov. 15, 1865.

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Note on ζ Herculis. By T. W. Burr, Esq.

In the last number of Vol. XXV. of the *Monthly Notices*, Mr. Fletcher draws attention to the fact that the above star, which with his large refractor is now "absolutely single," was a few years ago measurable with a telescope of 4 inches aperture only. This reminded me that I had seen it double with even less aperture, and thinking the period of this observation might be interesting I have referred to my note-book, and find that on the 19th May, 1853, I saw the pair well separated with a power of 480 on my Ross Equatoreal of $3\frac{3}{4}$-in. aperture, and that even with 173 the duplicity could be detected.

I did not apply the micrometer then, but some measures by Mr. Miller in the *Notices* for the same month gave $1''{\cdot}4$ for the distance.

Islington, Dec. 7, 1865.

Physical Constitution of the Sun. By Lieut. E. D. Ashe, Director of the Quebec Observatory.

The author states that his observations were made with a telescope of 8 inches aperture, 9 feet focal length, mounted equatoreally, driven by clock-work, and being one of Alvan Clark's superior telescopes. The power generally used was 400. He also states that the atmosphere of Quebec for astronomical observations is not second to that of Italy. He remarks that there are difficulties to overcome in the cavernous view of the question that he thinks are insurmountable.

He explains his own theory as follows:—I conceive the Sun's surface to be composed of an equally bright and incandescent photosphere, that our Sun is a nebulous star, and that the nebula consists in what is seen as the zodiacal light; that the spots are small meteor planets that revolve round the Sun, and that they fall into it; at first they are not visible even to the largest telescopes, but after they meet and spread out they cover comparatively a large surface. When they crack and split into pieces they show the bright surface of the Sun called bridges; that the penumbra is the dross or thinnest portion of melted matter, which when further attenuated breaks up into patches, and finally forms the darker portions of the granulations that cover the surface of the Sun. The faculæ are nothing more than the disturbance of this scum, showing the brighter portion of the Sun's surface, which disturbance must take place in the neighbourhood of these boiling, melting masses; also, that there is a drift, as mentioned by Mr. Carrington, which

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carries the faculæ away and distributes the scum over the entire surface of the Sun. And he adduces arguments, supported by references, that may sustain his view of the question.

M. Chacornac, in a letter dated 24 October, 1865, addressed to the President of the Society, refers to the continuation of his researches on the physical constitution of the Sun; and states that after various observations he has arrived definitively at the conclusion that the Sun is at least as luminous at its centre as in the brilliant envelope which bounds its visible contour ("que le Soleil est au moins aussi lumineuse à son centre que dans cette enveloppe resplendissante qui limite son contour visible").

Observations of Double Stars. By the Rev. Padre Secchi.

July 20, 1865. I measured the star *δ Cygni*, and found $P = 348°{\cdot}45$, $D = 1''{\cdot}465$.

July 20, 1865. *γ Coronæ* is round only occasionally; it seems there is a notch in direction 85°.

ξ Herculis likewise is round; in the best moments a notch appears in the disk on Dir. 315°.

51 *ξ Libræ*, A.B., Pos. = 155°, Dist. $0''{\cdot}40$, disks well separated.

Rome, Nov. 24, 1865.

The Society has received from Mr. A. Brothers a sheet of photographs of the Moon, taken during the Eclipse on the 4th Oct. with his Equatoreal telescope of 5 inches aperture. Mr. Brothers writes that the prints must not be looked upon as photographs of the Moon, as many very much superior have been taken, but merely as pictures of the Eclipse. The atmosphere was so much disturbed during the whole time of the Eclipse that the sharpness of detail is lost to a great extent. He attempted to obtain the entire outline of the Moon, but failed to get more than greater sharpness of the shadow; this will be seen in No. 10, which was exposed 15 seconds; Nos. 8 and 12 were exposed 3 seconds, and the remainder from 1 to about 2-tenths of a second.

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Observations of Encke's Comet. By G. R. Smalley, Esq.

(*Extract of a Letter addressed to the Astronomer Royal.*)

I send the approximate places of Encke's Comet, deduced from four sets of observations (such as they were) in June and July last. There were but four nights when I could see the Comet at all, and then observation was most difficult, and, from the absence of any appearance of condensation, bisection almost impossible.

Hitherto I have had no means of identifying the stars of comparison, and the results I send you are corrected for refraction and instrumental errors only.

Approximate Places of Encke's Comet (corrected for refraction), 1865.

Date.	Sydney Mean Time.	R.A.	South Declination.
	h m s	h m s	° ′ ″
June 28	7 14 6·9	8 26 19·8	10 49 2·0
29	7 4 59·0	8 34 59·3	12 58 54·5
30	7 18 39·7	8 44 14·5	15 14 56·5
July 1	6 51 0·6	8 54 13·3	17 32 36·6

Comet very faint throughout.

No appearance of any nucleus or tail—a mere faint patch of haze.

Government Observatory, Sydney,
Sept. 22, 1865.

Occultations of Stars by the Moon, observed at Forest Lodge, Maresfield. By Capt. W. Noble.

Monday, July 3d, 1865.

8 *Libræ.*

The star disappeared instantaneously at the Moon's dark limb

at $16^h\ 33^m\ 50^s{\cdot}3$ L.S.T. = $9^h\ 46^m\ 8^s{\cdot}7$ L.M.T.

and reappeared at the bright limb

at $17^h\ 35^m\ 48{\cdot}4 \pm$ L.S.T. = $10^h\ 47^m\ 56^s{\cdot}7 \pm$ L.M.T.

α^2 *Libræ* disappeared instantaneously at the Moon's dark limb

at $16^h\ 47^m\ 26^s{\cdot}8$ L.S.T. = $9^h\ 59^m\ 43^s{\cdot}0$ L.M.T.

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and reappeared also instantaneously at the Moon's bright limb

at $17^h\ 39^m\ 13^s{\cdot}0$ L.S.T. = $10^h\ 51^m\ 20^s{\cdot}7$ L.M.T.

At the times of emersion the bubbling and boiling of the air was very great, and this renders the reappearance of 8 *Libræ* slightly uncertain.

Power employed 135 upon the micrometer, with of course a positive eye-piece. It was adjusted upon the stars.

Saturday, November 4.

δ^x *Tauri.*

The disappearance was not observed; but the reappearance took place instantaneously at the Moon's dark limb

at $1^h\ 34^m\ 4^s{\cdot}5$ L.S.T. = $10^h\ 37^m\ 21^s{\cdot}8$ L.M.T.

Power 255 adjusted on the star.

Sunday, November 5.

115 *Tauri.*

The star disappeared somewhat sluggishly at the Moon's bright limb

at $1^h\ 19^m\ 17^s{\cdot}9$ L.S.T. = $18^h\ 8^m\ 43^s{\cdot}4$ L.M.T.

The reappearance was not observed.

Power 255 adjusted on the star.

In each case the telescope employed was my Ross Equatoreal of 4·2 inches aperture and 61 inches focal length.

Forest Lodge, Maresfield, Uckfield,
November 9, 1865.

Observations of the Solar Eclipse, October 19, 1865.
By Lord Wrottesley.

The commencement of the Solar Eclipse of the 19th of this month was here observed to take place at $4^h\ 2^m\ 3^s$ Wrottesley Mean Time. Instrument, the 11-feet Equatoreal; observer, Mr. Hough.

The observation was not very satisfactory, as the eye of the observer was not directed to that part of the limb of the Sun, on which the first impression was made, at the instant of

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contact; but Mr. Hough is satisfied that the time above given is not more than 6ˢ too late.

The Sun was at times obscured by clouds, and the limbs of both Sun and Moon were exceedingly tremulous. The two great spots near the edge of the limb added much to the interest of the phenomenon.

Wrottesley, Oct. 29, 1865.

Notice of a recent Memoir of Prof. Krueger, of Helsingfors, on the Star-cluster in λ Persei. By R. C. Carrington, Esq.

Prof. Krueger has recently sent copies to this country of the memoir above named, and accompanying my copy has written me a request that I would call the attention of the Society to a conclusion to which his researches have led, and which he considers to require re-investigation by an independent person. Regarding the example of Bessel in very carefully recording the relative positions of numerous principal components of the Pleiades as one to be followed in other similar cases, he has in similar manner by heliometric observations ascertained, and placed on record the relative positions of 43 stars in λ *Persei*, by referring them in distance and position to a double star *d* which he took for his zero-point; and a catalogue and chart and particulars of reduction and theory are appended. He has further divided his results into two groups of Spring and Autumn, results between which a difference comes out which would (if no instrumental influence vitiates the conclusion) imply a position parallax of the cluster of $\frac{2}{3}$rds of a second as referred to the double star, or that the double star *d* the components of which are of the ninth magnitude is situated considerably behind the cluster of which optically it appears a component. As there is an inherent improbability attached to this conclusion, Professor Krueger suspends his judgment till he is able to further examine the question, and lays the result before others as an interesting case for independent investigation.

It needs no remarks from me in addition to his own to urge the desirability that this should speedily be done by those who possess the opportunities and necessary qualifications for so delicate a research; but I would take the opportunity of remarking that several other clusters offer similar fields of useful labour to those members of the Society who, though not prepared to enter on researches requiring many continuous years of application, are yet willing to take up subjects of more moderate extent.

Dec. 1865.

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The Time-signalling Operations at Glasgow. By Prof. Grant.

The President at the conclusion of the Meeting, called upon Professor Grant of Glasgow, who was present, and who has been making some successful experiments in distributing time over the city of Glasgow, to give an account of his operations.

Professor Grant stated that, in the time-signalling operations at Glasgow, Jones's method of regulating clocks is exclusively used. It is generally known that, according to this method, the electric fluid is employed merely as a regulating agent, and not in any case as a motive power, the time-piece under control being an ordinary clock, connected by a regular succession of electric pulsations with the normal mean-time clock of the Observatory. The application of the invention in Glasgow has been perfectly successful. It has been employed under various forms; but what Professor Grant considered to be the most suitable to the requirements of a large city was the small clock with a seconds' pendulum and a dial of about 3 feet in diameter, showing the time to hours, minutes, and seconds. Clocks of this construction have been set up in the public thoroughfares of Glasgow, and have been found to be exceedingly useful. Attached to each of them is a galvanometer, which, by its deflections, gives an indication of the electric currents transmitted in successive seconds from the normal mean-time clock of the Observatory, and a break in the transmission of the currents, once in every minute, namely, at the sixtieth second, of the Observatory clock, supplies the public with an unerring criterion for testing the accuracy of the controlled clock. There were now eleven clocks of various forms in Glasgow under the electric control of the mean-time clock of the Observatory. In a short time the number would be increased to some seventeen or eighteen, and the system was gradually extending over all Glasgow. The going of these clocks was truly marvellous. From week to week and from month to month they continued to indicate the time with the utmost precision, occasioning merely a little attention now and then to the battery power. It was one of the advantages of Jones's method of control that, even in the case wherein the operations were on an extensive scale, only a small amount of battery power was necessary. There was one other remark which he would make, and it had reference to turret-clocks. Hitherto it had been usual, in the operations for placing one of such clocks under control, to remove the two seconds' pendulum, and to substitute for it a seconds' pendulum, which was made to beat in exact unison with the pendulum of the Observatory clock. Objections to this practice have been expressed by many persons who consider that a heavy pendulum vibrating once in two seconds is much better adapted than a light seconds' pendulum for maintaining the steady going of a

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clock fitted up in a lofty tower, the dials and hands of which are necessarily exposed very much to the action of high winds. After a good deal of experiment, Professor Grant found that the two seconds' pendulum might be retained and kept under complete control by attaching a larger wire coil to the bob, and using a more powerful system of magnets in combination with it.

Mr. Pritchard: Do you mean that you control at once pendulums that beat a second and pendulums that beat two seconds by the same wire?

Professor Grant: Yes. We have two turret-clocks in which the long pendulum has been removed and the short one substituted; but we have a third one in which the long pendulum has been retained, and the result has been completely successful. Nothing can be more satisfactory. It goes on from week to week without any trouble, and the result is as exact as in the case of the smaller clocks.

Letter to the President from Dr. Donati, Director of the Observatory, Florence.

"*Florence, le 27 Dec.*, 1865.

"Si l'on compare les éléments, que j'ai calculé pour la Comète que le Rev. P. Secchi annonça d'avoir trouvée le 9 courant, avec les éléments de la Comète de Faye, on s'aperçoit que la Comète annoncée par le P. Secchi n'est autre chose que la Comète de Faye.

"Peut-être vous avez déjà fait cette remarque, que je n'avais pas faite d'abord, en partie à cause de l'annonce donnée par le Rev. P. Secchi, et en partie parcequ'il était réellement peu presumable que dans ces jours on pût observer de nouveau la Comète de Faye."

ERRATA.

Vol. XXV., p. 56, end of line 13, and lines 15, 20, 23, and 25, and p. 57, line 10, *for* α *read* 2α.

Vol. XXVI., No. 1, page 34, line 11, *for* which surpassed, *read* that of Mr. Rutherford surpassed.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *January* 12, 1866. No. 3.

WARREN DE LA RUE, Esq., President, in the Chair.

Thomas G. Rylands, Esq., Heath House, Warrington;
Rev. A. W. Deey, Alton, Hants;
Frederick Robert Hughes, Esq., Borrowstownness, N.B.;
The Hon. John William Strutt, Tirling Place, Witham, Essex; and
Capt. Charles Thomas Curme, R.N., Milborne Port, Somerset,

were balloted for and duly elected Fellows of the Society.

Proposition for a Telescope on the Andes. By Lieut. E. D. Ashe, Director of the Quebec Observatory.

(*Letter to the President.*)

As President of the Astronomical Society, I venture to suggest to you that a first-class telescope should be placed on one of the higher Llanos of the Andes, some of which are admirably adapted for astronomical observations; and as the Geographical Society has had no difficulty in raising funds for several of their praiseworthy expeditions, neither do I anticipate any difficulty in defraying the expenses of an astronomical party, whose object would be to observe the discs of planets and the surface of the Sun.

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Much was done by the Teneriffe expedition; still incomparably greater results might be expected from placing a glass at a much higher elevation, and removed from the effects of a moist atmosphere.

It was my good fortune, when belonging to a frigate on the South American Station, to have crossed the Andes, in Peru, in about Lat. 18° South.

The Pass of Tarcora has an elevation of about 20,000 feet, and is reached easily in three days from the delightful city of Tacna, situated on the coast.

From the Pass to the Lake of Titicaca are numerous plains or Llanos, at different heights, varying from 12,000 to 20,000 feet; one of which might be selected for the purpose. I should be very happy to give any information or render any assistance.

As a rough sketch of the route of the expedition to its destination, I may say that the party should go by the West Indian Mail to Panama; there a man-of-war steamer would be ready to take them to Arica, on the coast of Peru, and from there the expedition would proceed to the pretty town of Tacna, distant forty miles, where it could remain and enjoy itself, and send out surveying parties, who would bring back information, upon which it would select a spot, and proceed to its destination.

There would be no difficulty in conveying the different parts of an Equatoreal, as a mule can carry 4 cwt.; and as I have seen a "Collard and Collard" pianoforte upon a mule's back crossing the Andes, there can be no doubt that the different parts of the telescope could be carefully carried.

No one need join the party who could not stand a little roughing, and endure the fatigue of thirteen hours upon the back of a mule for several days together.

I enclose an account of my journey across the Andes, which may give some information to those desiring to join the expedition; and, in conclusion, I may say that it is probable that the Canadian Government would allow my telescope of 8 inches aperture and my services upon so laudable an undertaking.

London, November 10, 1865.

The President: I have since received a letter from Commander Ashe, and he very strongly urges the desirability of an expedition to Peru. He has a very high opinion of the atmosphere there, and thinks that we should obtain observations such as we could get in no other part of the world. The letter is of value, from its containing the experience of a man accustomed to make astronomical observations.

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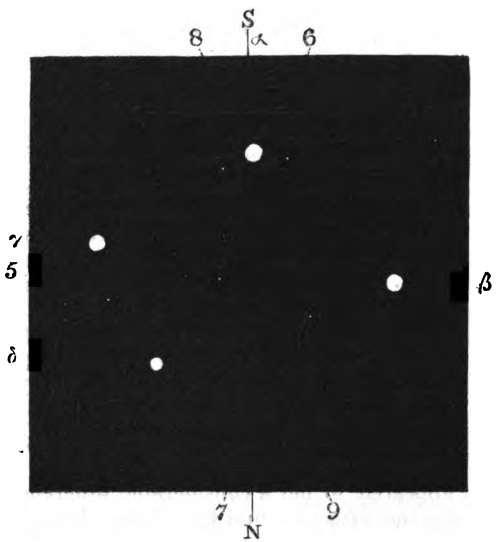
Mr. Evan Hopkins, who has travelled in Peru, bore testimony to the excellence of the climate.

On the Stars within the Trapezium of the Nebula of Orion.

By William Huggins, Esq., F.R.S.

Since a prismatic analysis of the light of the great Nebula in *Orion* has shown that this remarkable object has a gaseous constitution, the changes in the arrangement of the luminous matter and in the relative brightness of different parts, which some observers are of opinion are taking place in this nebula, possess great interest and importance. For it will be probably from a more careful and systematic observation of the alterations in form and brightness which may take place in this



The Trapezium of Orion, as seen by Mr. Huggins, Jan. 6 and 8, 1866.

object, and in the other gaseous nebulæ, that the necessary evidence will be obtained for a future determination of the true cosmical rank and relations of these bodies.

The discrepancy which exists between the published observations of the minute stars within the trapezium and in its neighbourhood, appears to show that the stars are subject to considerable variation in brightness.

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M. Otto Struve remarks, "We have amongst the six observed stars in the Regio Huygheniana, on a space of three to four square minutes, at least four variable stars; but I think the number of variable stars in the same region will yet be considerably increased by further observations. The sixth star of the trapezium bears strong indications of variability. . . . Probably two or three stars, seen and measured by M. Lamont in 1837, will be found of variable light. . . . Perhaps some of the stars indicated by De Vico, Bond, and others, in the immediate vicinity of the Trapezium, might be found of variable light."*

On the evening of January 6, 1866, the atmosphere was unusually favourable for the detection of minute points of light, and I was able to estimate satisfactorily the positions and relative brightness of three very small stars situated within the space enclosed by the well-known six stars of the Trapezium. They were detected with a Kellner eye-piece magnifying 60 diameters, and were afterwards seen with double-convex lenses, magnifying 135 and 220 diameters. The observations of January 6 were confirmed on January 7 and 8.† The positions of these minute stars relatively to the other stars of the trapezium are shown in the diagram which accompanies this paper.

The fifth and sixth stars were steadily seen. In 1852 Mr. Lassell obtained some fine views of these stars, and he puts on record that "the sixth star seemed equally bright with the fifth, and quite as easily seen."‡ On December 8, 1856, Mr. Lassell remarks, "The sixth star appears decidedly the brightest, or the largest, intrinsically, and if in the place of the fifth would I think be more easily seen. If these stars are unaltered in magnitude I cannot understand why I should, many years ago, have so often scrutinized the fifth star with this telescope before I detected the sixth."§ In 1857 M. O. Struve says, "On former occasions I agreed perfectly with Mr. Lassell's remark that the sixth star was fully of equal brightness with the fifth star; but this year it appeared to me constantly inferior."‖

There appeared to me a considerable difference in brightness between the fifth star and the sixth. Frequently during the passage of masses of haze across the stars the fifth was seen steadily when the sixth could not be detected. My observations agree with the magnitudes assigned to these stars by Sir John Herschel in 1834. Sir John Herschel says, "Applying the aperture 12 inches, the sixth star was finely seen. It is ex-

* *Monthly Notices*, vol. xvii. p. 227.

† On this evening my friend, Mr. S. B. Kincaid, visited the Observatory, and saw all the stars figured in the diagram.

‡ *Monthly Notices*, vol. xiv. p. 74. § Ibid. vol. xvii. p. 68.

‖ Ibid. vol. xvii. p. 227.

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cessively minute and very close to α, much more so than the fifth is to γ, say half the distance, and 14th mag., while the fifth = 12th mag."*

The minute star nearly in the middle of the Trapezium, and marked 7 in the diagram, appeared to me much fainter than the sixth star, probably nearly as much so as the sixth is now smaller than the fifth. I do not know whether a star has been seen before in this position within the Trapezium.

The star numbered 8 in the diagram is probably in a small degree brighter than 7, but the nearness of 8 to the bright star α makes the steady vision of it very difficult. This star can scarcely be the same as the one which M. Porro saw in 1857. In M. D'Abbadie's plan of the Trapezium, M. Porro's star is placed nearly halfway from α to γ.†

The star 9 of the diagram is rather feebler than 7. I was able to see it only by occasional glimpses. I should think there can be little doubt that this is one of the stars observed by De Vico in 1839.‡ I had recorded its place before I saw De Vico's diagram. The position which I have given to it is not identical with that of De Vico's star, but the extreme difficulty of this object made the determination of its position a little uncertain.

No one of the minute stars which I observed agrees in position with the "new star" discovered by Mr. Lassell at Malta on January 29, 1862. A diagram of this star is given in the *Monthly Notices*.§

We do not know whether these minute stars are physically connected with the great nebula, or are only in optical association with it. As a means of obtaining information on this point, it appears to be of great importance to determine whether the variability of the stars in the nebula and the occasional appearance of new stars can be associated with any changes which may be observed in the nebula.

Captain Noble: My friend Mr. Brodie has informed me that by gauging the fifth and sixth stars by Dawes's method of diaphragms he has found that the sixth star was, on the night he tried it, of precisely half a magnitude less than the fifth. I have seen the sixth star with my $4\frac{2}{10}$-inch aperture, and therefore, from what Mr. Huggins says about variability, I think the thing is put out of the range of hypothesis, and is almost

* *Cape Observations*, p. 30.

† *Monthly Notices*, vol. xvii. p. 245.

‡ "Memoria intorno a parecchie Osservazioni fatte in Collegio Romano, anno 1839."

§ Vol. xxii. p. 164.

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a certainty. I have tried many times to see that star, and could not, and yet I have seen it at others:—the fifth I can always see.

The President: I quite agree that the whole system of the trapezium with the included faint stars is variable. At times I have seen the sixth star very much brighter than the fifth, notwithstanding its proximity to its companion star, and I have also caught sight of the fainter stars, stars within the trapezium, without noting down their positions at the time. I believe those stars are variable because on other occasions I have searched for them in vain, and I think this will be found to be the case. Whether one can establish a physical connection between these various stars it is difficult to say, for some of them are so extremely difficult to be seen, and their variability renders it still more difficult to say whether it is the same star which is seen on different occasions. Diligent research and observation with respect to the nebula of *Orion* will certainly bring to light some interesting phenomena. I would also recommend the close observation of other groups of faint stars; for in many of these—I did observe them years ago more than I do now—I have noticed traces of variability.

Note regarding the Decrease of Actinic Effect near the Circumference of the Sun, as shown by the Kew Pictures. By Messrs. Warren De La Rue, Stewart, and Loewy.

The remarks which we ventured to make in the last paragraph of our Results on Solar Physics, recently published, have induced us to examine the Kew Pictures, as regards the decrease of actinic effect from the centre to the circumference of the Sun, to which decrease we may give the name of *atmospheric effect*, since it is without doubt caused by the presence of a comparatively cold solar atmosphere.

In conformity with our views, this atmospheric effect ought to be greater at the epoch of maximum than at that of minimum spot frequency; and furthermore, if there is any reference to ecliptical longitudes in the behaviour of spots—that is to say, if at any time the spots on the Sun attain their maximum at any ecliptical longitude, there ought (according to these views) to be a greater amount of absorbing atmosphere at the same longitude, since such an atmosphere is supposed conducive to the outbreak of spots.

There is reason to think that spots attain their maximum in the ecliptical longitude opposite to that where *Venus* exists, so that we might expect (according to these views) a diminution

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in atmospheric effect in the same longitude as *Venus*, and an increase in their effect in the longitude opposite to *Venus*.

If, therefore, *Venus* be at the longitude of the left limb of the Sun, this limb should exhibit less atmospheric effect than the right limb, and if *Venus* be at the right limb we should have most atmospheric effect at the left limb.

It is only the under-exposed pictures that are available for a research of this nature, since an over-exposure tends to do away with the atmospheric effect.

Without giving any hint of our views, Mr. Beckly was requested to select some of the last Kew pictures taken in 1859, a year of maximum spot frequency, and to compare them with those taken in 1864 and 1865, periods of minimum spot frequency; and he came to the conclusion that there was more atmospheric effect in 1859 than in the years 1864 and 1865.

Furthermore, Mr. Stewart, in conjunction with Miss Beckly, has looked over all the pictures taken at Kew from May 1863 to the present date; and this examination was made in such a way that the results could not derive any bias from the opinion of Mr. Stewart, as one of the joint authors of the *Researches on Solar Physics*, above alluded to; for whenever the two observers disagreed, the picture was referred to a third person.

The results of this investigation are given in the following table, and they are at least in conformity with our views, and not antagonistic, while at the same time the evidence is not sufficiently strong to establish conclusively the truth of an hypothesis.

1863.	Left.	Right.	Equal.	
May	2	2	1	Venus from 80 to 20 degrees to the left.
June	2	1	2	,, 50 to 60 ,, ,,
July	0	7	2	,, 30 to 40 ,, ,,
August	3	3	1	,, 10 to 20 ,, ,,
September	3	0	2	Venus in conjunction.
October	1	0	2	Venus about 5–10° to the right.
November	1	0	3	,, ,, 20° to the right.
December	2	2	1	,, from 30° to 40° to the right.
1864.				
January	4	0	0	,, about 45° to the right.
February	–	–	–	
March	3	1	0	,, ,, 90° ,,
April	3	3	4	,, ,, 120° ,,
May	–	–	–	Approaching opposition.
June	1	1	7	Very near opposition.
July	4	3	7	Venus in opposition.
August	1	0	2	
September	3	4	2	About 150° to the left.
October	0	4	3	,, 140° ,,

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1865.	Left	Right.	Equal.	
November	0	2	3	About 120° to the left.
December	0	1	2	,, 90° ,,
1865.				
January	0	2	0	
February	0	1	0	
March	4	3	3	
April	0	2	11	
May	1	1	10	Venus in conjunction.
June	2	0	1	
July	2	0	3	
August	6	1	5	
September	5	1	8	
October	8	2	5	Venus 90° to the right.

It thus appears, from the above table, which is the result of a joint and careful investigation of the Kew pictures by Miss Beckly and Mr. Stewart that

(1). When Venus is considerably to the left, there is most atmospheric effect to the right.

(2). When she is in conjunction or opposition, there is a tendency to equality.

(3). When she is considerably to the right, there is most atmospheric effect to the left.

A Comparison of the Kew Results of Observations on Sun-Spots with those of Hofrath Schwabe, in Dessau, for the year 1865. By Messrs. Warren De La Rue, Stewart, and Loewy.

	Kew.				Dessau.			
Months.	New Groups.	Numbers.	Days Observed.	Days without Spots.	New Groups.	Numbers.	Days Observed.	Days without Spots.
January	12	632 to 643	5	0	12	1 to 12	23	0
February	10	644 to 653	7	1(?)	11	13 to 23	16	0
March	10	654 to 663	16	1(?)	8	24 to 31	15	1(?)
April	8	664 to 671	15	0	12	32 to 43	30	0
May	13	672 to 684	19	2	10	44 to 53	31	0
June	5	685 to 689	10	1(?)	6	54 to 59	30	1
July	7	690 to 696	13	1	6	60 to 65	31	1
August	8	697 to 704	15	0	7	66 to 72	31	0
September	7	705 to 711	22	2	7	73 to 79	30	7
October	3	712 to 714	15	3	3	80 to 82	31	11
November	5	715 to 719	10	1	6	83 to 88	23	3
December	5	720 to 724	6	1	5	89 to 93	16	2
Total	93	632 to 724	153	13 3(?)	93	1 to 93	307	26

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Remarks.—The numbers in the Kew list form a continuation of the Catalogue of Groups, published in the first series of *Researches on Solar Physics*, by Warren De La Rue, Balfour Stewart, and Benjamin Loewy, pp. 6–8.

Those days on which it is doubtful whether spots were on the Sun or not, have the sign (?) after them.

The President stated that the Kew photographs are now taken by Miss Beckly, the daughter of the mechanical assistant of Kew; and it seems to be a work peculiarly fitting to a lady. During the day she watches for opportunities for photographing the Sun with that patience for which the sex is distinguished, and she never lets an opportunity escape her. It is extraordinary that even on very cloudy days, between gaps of cloud, when it would be imagined that it was almost impossible to get a photograph, yet there is always a record at Kew.

Mr. De La Rue remarked that all these investigations on the solar spots occupy a considerable time, and that the results may be interpreted differently by different persons. All that we have to do is to record faithfully the result of our observations; and it is hoped the Kew photo-heliograph will conduce very much to the advance of solar physics. Some time ago some experiments were made by the speaker, in taking solar spots on a very large scale, the pictures of the Sun's disk being on a scale of 3 feet for the Sun's diameter. There are certain difficulties in taking those pictures by means of a reflector; but Mr. Cooke has recently undertaken the construction of a 13-inch refractor, which it is intended to apply to solar and lunar photography.

On a New Method of Mounting Silvered Glass Specula and Diagonal Mirrors in Reflecting Telescopes. By John Browning, Esq., F.R.A.S.

Owing to their cheapness and portability, telescopes constructed with Foucault's silvered-glass mirrors are receiving a constantly increasing share of attention.

The focal length of these mirrors needs not exceed eight times their diameter, while for each inch in diameter—when of the best figure—they will bear a magnifying power of 100 on close stars in finest states of air.

The principal difficulty hitherto encountered in fitting-up these mirrors, when they are of large size, has been that of mounting them in such a manner as to avoid flexure. Various contrivances have been employed for this purpose; among

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others they have been supported on air-cushions, on layers of felt, and on a number of triangles, each triangle supported on a centre. All these plans appear to me to possess the common fault, that a blow, or shock of any kind, or even a change in the position of the telescope, frequently throws the speculum out of adjustment. The method I have devised for overcoming this difficulty I shall now proceed to describe.

First, however, I wish to state that I have the mirrors made of glass much thicker than is generally employed for the purpose. For a speculum 6½ inches in diameter I use glass of a thickness of 1 inch, for a speculum 10 inches in diameter glass 1½ inches in thickness. The extra cost of the glass, as it is only ordinary plate-glass, is merely nominal, and the difficulty of working the mirror not at all increased.

After the disk which is to form the mirror is shaped, but before the parabolic figure has been given to it, I work the back carefully to a very perfect plane, by the same method I employ for working the plane surfaces of prisms. When this has been done the mirror has the parabolic figure given to it.

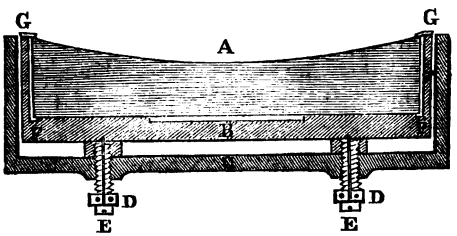

Fig. 1.

A cast-iron cell (B, fig. 1) is now prepared, whose internal diameter is rather larger than the mirror. The bottom of this cell when intended for a 10-inch mirror should be at least three-quarters of an inch thick.

The inside of this cell, on which the mirror will have to rest, I also bring carefully to a plane surface in the following manner. I first turn it with a slide rest in a lathe in the usual way. Then I prepare a Whitworth's plane surface of a circular form, one-eighth of an inch less in diameter than the mirror to be mounted. The bottom of the cell is made flat by scraping, being tested repeatedly during the process with the circular Whitworth's surface.

It greatly facilitates this operation if a grove be turned in the bottom of the cell, at F F, in diagram 1. The centre of the bearing surface of the cell, to the extent of one-third, may be advantageously turned away. This materially lessens the risk of the mirror being deflected, either by the presence of any particles of foreign matter or by the cell altering its form owing to changes of temperature.

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For this suggestion I am indebted to the kindness of Colonel Strange. Before the mirror is placed in its cell, the bearing surface is slightly smeared with the best oil, to prevent the oxidation of the iron. Lastly, the mirror is secured in its position by a ring shown at G G in diagram 1.

In adjusting the mirror it is necessary to avoid placing any strain upon the cell, as a very small strain is sufficient to deflect the bottom of the cell, and this deflection extends to the mirror. This is avoided by placing the cell in another cell, and adjusting it in position by means of hollow screws, D D, E E, as shown in the diagram. These screws allow of the cell being raised or lowered without throwing any strain upon the cell. The silvered surface of the mirror is protected by a tightly-fitting cover, which is adapted to the edge of the cell, B, which is turned conical.

Some time since Mr. With, of Hereford, so well known for the success with which he has given his attention to the construction of silvered-glass specula, suggested to me that their performance would probably be improved if some other method were adopted of mounting the diagonal reflector or prism.

As at present mounted on an arm, they are subject to vibration, and the substance of this arm produces coarse rays on bright stars.

I have endeavoured to obviate these inconveniences by the plan of mounting represented in the diagrams figs. 2 and 3.

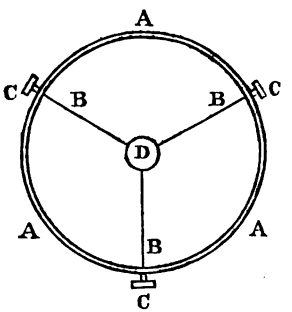

Fig. 2.

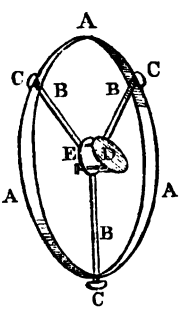

Fig. 3.

In these diagrams fig. 3 is a perspective drawing, and fig. 2 a front view of the arrangement.

The letters are the same for the same parts in both diagrams. D represents the diagonal mirror; B B B, three pieces of chronometer spring stretched tightly, by means of screws through the ring, A A A. This movement is available for centreing. The diagonal mirror or prism, D, is attached by three pillars to a round plate, E, whose diameter is the minor axis of the ellipse. The adjustment is made by means of

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hollow screws, of the same kind I have referred to in the commencement of this paper, as applied to the speculum.

The most difficult tests for silvered-glass specula are very bright stars of large magnitude. Their performance on this class of objects is considerably improved by using a good Barlow lens of 4 or 5 inches virtual focus. At the same time, as is well known, the employment of this lens forms a most convenient contrivance for increasing the power of the various eye-pieces. For low powers, achromatic eye-pieces made by combining two achromatic combinations, each consisting of a plano-concave flint and a double-convex crown cemented together, arranged so that the convex side of the crown lenses almost touch each other, answer well. When used in this manner, if the parabolizing operation on the speculum has been conducted with sufficient care and skill, the performance of an 8½-inch speculum will bear favourable comparison with a 7-inch refractor in point of light, and exceed it in separating power, while the expense need not be more than one-fifth of the cost of such an object-glass.

In conjunction with Mr. Slack, I have contrived a simple and very substantial form of equatorial mounting entirely in cast-iron for these telescopes. At the same time I am greatly indebted to Mr. Slack for the valuable assistance he has rendered me in carefully testing the results of the various modifications I tried in the course of my experiments on mounting specula, before I finally adopted the plans I have just described. With a plain glass mirror as a diagonal reflector, unsilvered, so as to allow the heat-rays to pass freely, these telescopes answer admirably for observations on sun-spots.

Telescopes provided with these mirrors are well adapted for spectrum observations on the stars. From their large aperture, as compared with their focal length, they give abundance of light, and they are not liable to any disadvantages on the score of the want of achromacy.

It is well known there is a considerable number of interesting observations which can only be made with large apertures, and that consideration of expense alone has hitherto prevented their being used.

A damp atmosphere is very prejudicial to the silvered surface of these specula. I have endeavoured to obviate this difficulty, and at the same time prevent the spiral-tube currents by covering the mouth of the telescope with a disk of glass, having plane and parallel surfaces. The production of these parallel plates is attended with considerable difficulty, but I have succeeded in applying one of 8-inches diameter to a mirror, apparently without injuring its performance, but I regret that I cannot speak upon the subject with confidence, as the long continuance of unfavourable weather has prevented me from making any satisfactory experiments.

I shall be highly gratified if the attention I have given to

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the mounting of these mirrors and telescopes should lead to their more general adoption.

The President: I think the most important part of the communication is the means of suspending the small mirror. The suppression of a great part of the thickness of the arm tends to do away with the rays which we all know appear to shoot out from the image of a star in the reflector. With respect to the mounting of the large mirror, I think that Mr. Browning's plan would answer very well up to 6 or 8 inches, but the moment one goes beyond that size there must be some such means of suspension as that adopted in Mr. Lassell's telescopes, or there must be flexure; no accuracy of fitting will prevent that.

Observations and Elements of the Comet of December 9, 1865. By Prof. Donati.

(*Translation of a Letter to the President.*)

I have determined the following positions of the Comet discovered by the Rev. Padre Secchi at Rome on the 9th December:—

1865.	Florence M.T. h m s	R.A. h m s	Decl. ° ′ ″
Dec. 11	7 9 19	22 50 18·44	+4 0 15·9
12	6 44 20	22 52 12·55	3 56 43·8
13	8 11 30	22 54 16·70	3 52 5·3
14	8 16 20	22 56 14·72	3 47 36·5
15	8 16 46	22 58 13·90	3 43 12·2
16	8 23 49	23 0 14·74	3 38 26·9
17	8 52 34	23 2 17·89	3 33 16·0
18	8 28 40	23 4 17·19	3 28 13·2
19	8 9 28	23 6 15·84	3 22 53·1
20	8 14 47	23 10 28·20	3 17 11·1
21	8 18 58	23 10 28·20	+3 11 12·5

The Comet is of extraordinary faintness. Using the observation at Rome of the 9th December, and my own observations of January 13 and 19, I have calculated the following elements:—

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T	=	1866, Jan. 20·32064 Greenwich M.T.			
		°	′	″	
☊	=	205	15	39	Mean Equinox, 13 Dec. 1865.
ϖ	=	31	22	31	
i	=	12	14	10	
log. q	=	0·28886			

This orbit satisfies exactly the mean observation, but it gives for the observation of Dec. 21 the following errors:—

Obs. − Cal.	
R.A.	Decl.
23″·7	+ 8″·9

Although the elements are only a rough approximation, we see that the orbit is quite different from that of the Comet of Biela, especially in the perihelion distance and in the position of the orbit in its plane. But the new Comet moves in a plane not very different from that of the Comet of Biela, and passes its perihelion nearly at the same time. These coincidences and the circumstance of not finding the Comet of Biela at its place may be the result of accident, or may have a physical reason. It seems to me that the mysterious phenomena which have been already remarked in Biela's Comet permit the asking of this question—which at present I cannot answer; but it may be hoped that science will give a solution of it.

Florence, 22 Dec. 1865.

Occultation of 115 *Tauri by the Moon.*

By C. G. Talmage, Esq.

I beg to enclose the times of the occultation of 115 *Tauri* on 1865, December 30:—

		h	m	s
G.M.T. of Disappearance = Dec. 30		7	34	55·60
,, Reappearance		8	21	54·35

Both times are exact, observed with a power of 85 on the 10-inch Equatoreal.

Mr. Barclay's Observatory, Leyton,
1866, *January* 11.

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Observations of η Argûs. By J. Tebbutt, Jun. Esq.

In accordance with the promise contained in my letter of the 17th July last, I now send you my observations of the remarkable southern star *η Argûs*. The comparisons have all been made with the naked eye, unless where otherwise stated, and under the most favourable conditions of sky, &c. The first comparisons were made in July 1854; but it appears from my journal that *η Argûs* was employed by me as a comparison-star on the 30th September and 12th October, 1853, in sextant observations of a large comet at that time visible in the morning sky. It was then a reddish star of the first magnitude.

1854, July 5. Of *α* and *β Centauri*, *α Crucis*, and *η Argûs*, the first was by far the brightest; *β Centauri* and *η Argûs* were very nearly equal, but I think the latter was somewhat the brighter. *η Argûs* somewhat exceeded *α Crucis*.

1860, May 4. 8 P.M. *η Argûs* was less than *β Canis Majoris* and about equal to *β Canis Minoris*. The standard stars were, however, too near the horizon for a good comparison, and the Moon was very bright. It was also about equal to *δ Crucis* and *θ Argûs*.

1860, May 18. $6^h\ 30^m$ P.M. About equal to *β Canis Minoris*, but not quite so bright as *β Canis Majoris*.

1862, January 26. Not so bright as *β Canis Minoris* or *θ Argûs;* perhaps slightly less than *σ Orionis*, and exactly equal to B.A.C. 3740.

1862, June 23. About equal to *ε Crucis* and B.A.C. 3526 and 3619, and considerably less than *δ Crucis* or *θ Argûs*.

1862, July 15. Greater than B.A.C. 3688 and less than B.A.C. 3740. The same result was obtained with a small telescope.

1862, July 22. *η Argûs* was of a magnitude intermediate between B.A.C. 3740 and 3688, and very little brighter than *ζ* and *η Crucis*.

1862, July 24. $9^h\ 23^m$. Slightly less than B.A.C. 3740 and 1818, and exactly equal to B.A.C. 3594.

1862, July 25. 6^h P.M. Less than B.A.C. 3740 and 3818, equal to B.A.C. 3594, and greater than B.A.C. 3655 and 3688.

1862, August 28. $7^h\ 20^m$ P.M. Greater than B.A.C. 3688; equal to B.A.C. 3594; and less than B.A.C. 3740.

1863, April 15. 10^h P.M. Greater than B.A.C. 3655 and 3688, and less than *ε Crucis* and B.A.C. 3740 and 3818. It was decidedly a shade less brilliant than B.A.C. 3594.

1863, April 16. 11 P.M. Somewhat less than ω^1 and ω^2 *Scorpii* and B.A.C. 3594.

1863, May 24. $8^h\ 15^m$ P.M. Less than *ε Crucis* and B.A.C. 3740 and 3818; greater than B.A.C. 3688; and about equal to

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ω^1 and ω^2 *Scorpii*. It was decidedly a shade less brilliant than B.A.C. 3594, which was very nearly equal to B.A.C. 3818.

1864, March 23. 9^h P.M. Equal to B.A.C. 3655 and 3688, both by the naked eye and the telescope. Full Moon.

1864, April 23. $6^h\ 30^m$ P.M. A shade less than B.A.C. 3655 and 3688; it was certainly not greater. It was brighter than B.A.C. 3642, which could only be seen by oblique vision.

1864, May 31. 7^h P.M. About equal to B.A.C. 5501, a small star a little north of *Antares*, and about equal to or perhaps slightly inferior to B.A.C. 3655 and 3688; it was superior to B.A.C. 3642.

1864, July 28. 7^h P.M. Somewhat inferior to B.A.C. 3655 and 3688, and slightly superior to B.A.C. 3642.

1865, February 22, 23, and 24. 9^h P.M. η *Argûs* was certainly inferior to B.A.C. 3655 and 3688; the stars were at a great altitude.

1865, March 14. $9^h\ 20^m$ P.M. η *Argûs* was distinctly visible to the naked eye, notwithstanding the Moon. It was certainly not quite so bright as B.A.C. 3655 or 3688.

Windsor, New South Wales,
October 19, 1865.

Elements of Comet I. 1865. By J. Tebbutt, jun. Esq.

I also avail myself of this opportunity to forward my corrected elements of the Great Comet of this year; they agree pretty well with the series of observations made at Melbourne and Windsor.

T = 1865, January $14^d{\cdot}32520$ G.M.T.

$$\left.\begin{array}{ll} \pi & 4\ 50\ 24{\cdot}4 \\ \Omega & 253\ 3\ 10{\cdot}4 \end{array}\right\} \text{M. Equinox, } 1865{\cdot}0.$$

i 92 28 20·0

$\log q$ = 8·4147561

Windsor, New South Wales,
October 19, 1865.

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There is contained in the *Comptes Rendus* of the French Academy, t. lxi. (December 1865), an interesting paper by M. Delaunay—"Sur l'Existence d'une Cause Nouvelle ayant une influence sensible sur la valeur de l'équation séculaire de la Lune." The substance of this paper is as follows:—

Assuming that the secular acceleration of the mean motion of the Moon, as indicated by observation, is notably greater than that occasioned by the variation of the excentricity of the Earth's orbit, it becomes necessary to seek for a new cause for the excess in question, or portion not accounted for by Laplace's theory. A progressive cooling of the terrestrial globe would produce a diminution, not an increase, of the acceleration, and the effect is therefore not to be so accounted for. But M. Delaunay considers that he has found a cause to which it is natural to attribute the excess in question, a cause, that is, which produces a progressive retardation in the rotatory motion of the Earth, and consequently an apparent acceleration in the mean motion of the Moon.

We know that the Moon by its action on the waters of the ocean occasions therein an oscillatory motion constituting the phenomenon of the tides. The Sun concurs in the production of this phenomenon, but, to avoid complication, the solar action may be disregarded. The form of the surface of the sea changing continually, it follows that the action of the Moon on the entire mass of the Earth (including therein the waters of the sea) is at every instant somewhat different from what it would be if the phenomenon of the tides did not exist; in seeking to explain wherein consists the difference, it is seen that *it consists principally in a couple acting constantly on the Earth in a direction contrary to the rotatory motion*, and that the result is a progressive diminution of the angular velocity of the terrestrial globe.

To further explain this, imagine, in the first instance, that the Earth is entirely covered by the waters of the ocean. In virtue of the action of the Moon, the waters tend to elevate themselves above their mean level in the two opposite regions situate at the extremities of the diameter directed towards the centre of the Moon. Admitting for greater simplicity that, without this action of the Moon, the surface of the sea would be exactly spherical, and that the Moon is in the plane of the Equator, then, in virtue of the lunar action, the surface of the sea tends constantly to assume the form of an ellipsoid of revolution, having the same centre as the sphere, and its transverse axis directed in the line joining the centres of the Earth and Moon. The effect of the Earth's motion of rotation is that this ellipsoid moves in regard to the Earth exactly as the Moon in its diurnal revolution, since the axis of the ellipsoid is always directed towards the Moon. But this continual dis-

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placement causes that the surface of the waters is never coincident with that of the ellipsoid of equilibrium — the frictions and resistances which the waters suffer in their motion produce the effect that the axis of the elongated figure of the waters at any instant *lags* behind that of the ellipsoid of equilibrium wherewith it tends to coincide. Without this retardation, the high tide would take place at the instant of the passage, superior or inferior, of the Moon over the meridian; in consequence of the retardation, the high tide takes place, not at the instant of the passage, but some hours after it.

The phenomenon is more complicated in the actual case, where the terrestrial surface is not completely covered by the waters, but whatever the distribution of land and water may be, the frictions and resistances of every kind which the waters suffer in their motion produce an effect similar to that indicated in the first-mentioned case. There is always a retardation in the oscillatory movement. Speaking generally, the oscillatory movement is what it would be if these resistances did not exist, and if the Moon was at every instant in a position in the heaven *behind* the actual position, in regard to the direction of the apparent diurnal motion.

Returning to the more simple case where the surface is completely covered, let us consider how the action of the Moon is modified in consequence of the elongated form produced in the waters of the sea by this same action. There exist, as it were, two fluid prominences, situate at the extremities of a diameter directed, not towards the Moon itself, but towards a point in the heavens situate at a certain distance eastwardly from the Moon. These two prominences are at unequal distances from the Moon, the one nearer, the other further than the centre of the Earth. Bearing in mind the explanation of the lunar action which produces the tides, it is easy to see that the nearer prominence is, as it were, attracted to, the further prominence repelled by, the Moon. The resultant action is that of a couple applied to the mass of the Earth, and tending to make it rotate in the direction contrary to that of its actual motion—a couple which therefore tends to retard the motion of rotation.

M. Delaunay remarks in a note that this idea of the resistance which the Moon continually opposes to the rotatory motion of the Earth, in consequence of its action on the waters of the sea, has been referred to in several printed works; but that it has been always assumed that the effect is too small to be sensible. He recalls that the object of his communication to the Academy was not to make known this cause of retardation, but to show—1. That the resulting retardation is far from insensible; 2. That it is possible to see therein a complete explanation of the portion, not accounted for by Laplace's theory, of the lunar acceleration.

Returning to the discussion, and imagining to fix the ideas

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that the retardation of the time of high water is three hours, or, what is the same thing, that the diameter through the two prominences is inclined at an angle of 45° to the line directed to the centre of the Moon, let us calculate the effect which the couple produces, replacing the two liquid prominences by material points situate at the extremities of the oblique diameter in question.

Denoting by T the centre of the Earth; by L that of the Moon; by E the extremity of the equatoreal radius inclined at an angle of 45° to the line T L eastwardly; and by E′ the opposite extremity of this diameter; let M be the total mass, m the mass of the Moon, and μ that of each of the material points supposed to be placed at E and E′ respectively. Suppose moreover that R is the distance T L, taken to be constant, of the centres of the Earth and Moon, r the terrestrial radius T E or T E′; and d the distance of the centre of the Moon from the point E. Taking f as the unit of attraction, we have

$$\frac{fm\mu}{d^2}$$

for the attraction of the Moon on the material point of mass μ at E. To refer the motion of the terrestrial globe to axes fixed in direction through its centre of gravity, we must join to this a force $\frac{fm\mu}{R^3}$ acting along the line L T in the direction from L to T. We have, moreover,

$$d^2 = R^2 + r^2 - 2Rr\cos 45° = R^2 + r^2 - \sqrt{2}Rr.$$

The resultant of these two forces constitutes the relative action of the Moon on the mass μ at E: taking the sum of these moments in regard to the centre of the Earth, and neglecting smaller terms, we find $\frac{3}{2}\frac{fm\mu r^2}{R^3}$, for the expression of the moment in question.

The same expression gives also the moment in regard to the second mass μ at E′. The total moment produced by the attractions on the two masses μ at E and E′, tending to retard the motion of rotation, is therefore equal to $\frac{3fm\mu r^2}{R^3}$, and consequently the differential equation for the motion of rotation, ω being the angular velocity and I the moment of inertia of the Earth in regard to a diameter, is

$$\frac{d\omega}{dt} = -\frac{3fm\mu r^2}{IR^3},$$

and taking the Earth to be homogeneous, we have

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$$I = \frac{2}{5} M r^2.$$

Moreover, by considering the motion of the Moon about the Earth, and disregarding μ in comparison of M, we find $f = \frac{4\pi^2 R^3}{T^2 M}$, where T is the period of the sidereal revolution of the Moon about the Earth. Introducing the values of I and f, the differential equation becomes

$$\frac{d\omega}{dt} = -30 \frac{\pi^2}{T^2} \frac{m}{M} \cdot \frac{\mu}{M},$$

and, integrating twice, it appears that the total angle of rotation during the time t is, in consequence of this action of the Moon, less than it would otherwise be by a quantity A, the expression of which is

$$A = 15\pi^2 \frac{m}{M} \cdot \frac{\mu}{M} \frac{t^2}{T^2}.$$

Let us now inquire what is the value to be attributed to these masses μ, in order that the retardation A corresponding to a time t equal to a century, may give rise to an apparent acceleration equal to 6 seconds (which is about the value of the portion not accounted for by Laplace's theory). We must for this purpose suppose A equal to the angle described by the Earth in its rotatory motion, while the Moon advances 6 seconds in its mean motion round the Earth; A is therefore equal to 6 seconds multiplied by $27\frac{1}{3}$ or to 164 seconds.

Effecting the calculation, and taking $\frac{1}{80}$ for the ratio of the Moon's mass to that of the Earth, we find

$$\mu = \frac{1}{4160\,000\,000} M.$$

To gain a more distinct notion of the magnitude of this mass, assume that it is the mass of a volume V of water; then, if 5,5 is the mean density of the Earth, we find

$$V = 1429\,000\,000\,000 \text{ cubic metres},$$

or, if this mass of water of the volume V has the form of a stratum on a circular base of the uniform thickness of one metre, the radius of the base will be about 675 kilometers; that is to say, such a stratum applied to the surface of the Earth would occupy a breadth of about 12 degrees at the equator: the magnitude of this stratum of water is obviously

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comparable with those of the fluid prominences which the action of the Moon would produce in the hypothetical case in question.

In presence of the foregoing result, obtained, indeed, on an hypothesis very different from the actual circumstances, it is, as M. Delaunay considers, impossible not to admit that an analogous effect of a sensible magnitude is produced by the Moon on the waters of the ocean.

The Sun, which contributes to produce the phenomenon of the tides, though in a less degree than the Moon, should equally contribute to this progressive diminution of the rotatory motion of the Earth.

The author states as follows his general conclusion:—

The perturbing forces to which are due the periodic oscillations of the surface of the sea (phenomenon of the tides) in exercising their action on the fluid intumescences which they occasion, cause a progressive retardation of the movement of rotation of the Earth, and thus produce a sensible apparent acceleration in the mean motion of the Moon.

He remarks that the foregoing result is not in accordance with what Laplace has found in investigating the influence that the state of fluidity of the waters of the sea may have on the movement of the terrestrial globe considered as a whole. Laplace says formally that this state of fluidity of the sea does not affect the uniformity of the rotation of the globe (*Mécanique Céleste*, book v.) But it is to be remarked that to arrive at this conclusion, Laplace confines himself to quantities of the first order in regard to the perturbing forces considered by him. It was, therefore, impossible for him to find the retardation of the movement of rotation, the real existence whereof has just been established, since this retardation is evidently of the order of the square of the perturbing forces in question.

The exact calculation of the retardation due to the combined action of the Moon and Sun, would require a knowledge of all the circumstances of the tides as well along the shores as in mid-ocean. Such a direct calculation is impossible; the actual retardation can only be found indirectly by means of the lunar acceleration, as determined by observation; and this gives a new interest to the comparisons of the lunar tables with the ancient eclipses, in the view of thereby arriving at the true value of the lunar acceleration.

The President called attention to two very important works among the presents; one from Professor Hansen and the other from M. Delaunay.* The volume contributed by Pro-

* The above-mentioned memoir in the *Comptes Rendus*.—Ed.

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fessor Hansen relates to the Lunar Theory, and is the second volume of the important work undertaken by that distinguished physical astronomer. This volume will afford an opportunity of studying the theory of Hansen with regard to certain inequalities of the lunar motion, and of comparing his investigation of the lunar acceleration with that of Mr. Adams. The coefficient of the lunar acceleration obtained by Prof. Hansen is much higher — about double that obtained by Mr. Adams; and a short time back there was a great controversy on the subject of the correctness of the one or the other. However, it seems to be established that Mr. Adams' coefficient is correct; but there can be no question that it does not fit exactly with the records of the ancient eclipses. M. Delaunay now steps in opportunely and proposes to account for this difference by the assumption that there is a retardation of the period of rotation of the Earth produced by the action of the Moon on the waters of the Earth, and causing the Earth to rotate more slowly by about 100th of a second than it did 2000 years ago. Whether the hypothesis will or not be at once accepted as true, it is certain that everything which comes from M. Delaunay must be regarded with the highest respect.

Mr. Stone said as follows: — The question discussed by M. Delaunay in this paper is a most interesting and important on. To clear up my own ideas on the subject, I have tried the question in a slightly modified, but, I believe, in a strictly analogous form. Suppose the Earth a sphere; the Moon to move in the plane of the equator; the Earth surrounded by a small *solid* shell of the mean density of the sea; the outer surface of this shell to assume the form of a prolate spheroid. Then, if the shell moves in such a manner that its greatest axis always lags three hours behind the Moon, the same assumption as that made by M. Delaunay, we have a rough geometrical representation of the "lunar tides." The shell can only move in this constrained manner through the action of friction between the shell and the Earth. I have first determined the moment of friction required to produce the constrained motion of the shell, and then the dimensions of the shell required to produce a diminution in the Earth's velocity of rotation, such that from it there would result an apparent acceleration of $6''$ in the Moon's mean motion. I find that a high tide of only 2 inches would be sufficient to produce the required retardation of the Earth's velocity of rotation. My result closely agrees with M. Delaunay's; my mass is even larger than his. This is as it should be, from the distribution of the mass in my problem. I would, however, remark that the case here considered is essentially that of a *solid shell.* Every particle of this shell has to move around the Earth in each month. I fear that the motion of a fluid mass under like conditions would be so exceedingly different that it would be

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very difficult, if not impossible, to infer anything like quantitative results respecting the fluid case from the solid case of our problem. I cannot think that any mere comparison of the matter in the protuberant parts of the solid spheroid and fliud spheroid gives us sufficient information on this point. No doubt, when pushed to the second order of small quantities, and therefore in an accurate solution, some retarding effect would be produced in the case of a fluid mass moving with high water lagging behind the Moon. The mean positions of the oscillating particles of fluid would be bodily transferred. We should have for the relative velocities of the fluid mass perpendicular to the Earth's axes non-periodic terms, and from such terms there would arise a permanent frictional moment on the Earth tending to destroy its velocity of rotation; but the question is, to what extent would this be the case. I cannot see that M. Delaunay's paper gives us any information on this point. I cannot therefore accept it as a demonstration of a retardation of the Earth's rotation sufficient to produce sensible effects on the apparent secular acceleration of the Moon. Whether the action of the friction between the Earth and the sea, necessary to produce the observed retardation of the tides, does produce a sensible effect on the length of the day or not, will, I fear, have, for the present, to be decided by the agreement or non-agreement of the theoretical value of the secular acceleration of the Moon's mean motion calculated on the assumption of the invariability of the sidereal day, with the observed value. Whether we accept or not the results of M. Delaunay's paper, there can be no difference of opinion respecting its value in recalling attention to the effects of the phenomena of the tides on the Earth's rotation.

Instrument for Sale.

Achromatic Refractor, $6\frac{4}{10}$-inch aperture, $8\frac{1}{2}$-feet focal length, with complete equatoreal mounting, driven by clockwork. Mounting by Troughton & Simms; object-glass by Merz. Apply to the Assistant Secretary.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *February* 9, 1866. No. 4.

THE Annual General Meeting of the Society:
WARREN DE LA RUE, Esq., F.R.S., President, in the Chair.

Arthur Brewin, Esq., 2 Copthall Chambers; and
Arthur Finch, Esq., Roupell Park, Streatham Hill,

were balloted for and duly elected Fellows of the Society.

The President then said:—

Before proceeding with the business before us, I would, for a few moments, bespeak your attention. You will recall the great regret felt by us all on learning that the Medal awarded last year to Professor Bond, of the United States, did not reach that country until after his lamented decease.

You will, doubtless, therefore, be much gratified now to learn that Professor Bond, though he did not actually receive the Medal, was, some time before his death, made aware of the honour that had been conferred upon him, and even of the grounds on which the award had been made. For, being very desirous of not omitting the mention of any of his numerous works, I early in January transmitted to him a rough copy of my intended address, with a request that he would kindly point out any omissions. Although himself too ill to reply, he, by his friend Lieut. Safford, expressed his heartfelt gratification at this recognition of his labours, while at the same time he confirmed the general accuracy and completeness of my proposed account of his researches.

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Report of the Council to the Forty-sixth Annual General Meeting of the Society.

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness and Honorary.	Total Fellows.	Associates.	Grand Total.
December 31, 1864	163	264	24	5	456	49	505
Since elected ...	7	22	...	...	...	...	...
Deceased	−5	−3	−2	−1	...	−3	...
Name withdrawn	...	−1	...	...	...	...	...
Expelled	...	−1	...	...	...	...	...
Resigned	...	−7	...	...	...	...	...
Removals	2	−2	...	...	...	...	...
Dec. 31, 1865 ...	167	272	22	4	465	46	511

Treasurer's Report of Receipts and Expenditure for the year ending December 31, 1865:—

RECEIPTS.

	£	s.	d.	£	s.	d.
Balance of last year's account	...	...		281	10	11
By Dividend on £2100 Consols	30	14	3			
By ditto on £4700 New 3 per Cents	68	14	9			
By ditto on £2100 Consols ...	30	19	6			
By ditto on £5000 New 3 per Cents	73	15	0			
				204	3	6
On account of arrears of contributions ...	147	16	0			
164 contributions, 1865–6, 6–7	343	7	0			
9 compositions	189	0	0			
29 admission-fees	60	18	0			
21 first contributions	34	13	0			
				775	14	0
Sale of publications	...	...		73	13	0
Due to Assistant-Secretary — petty cash	...	...		0	1	1
				£1335	2	6

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EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:—						
Editor of Publications	60	0	0			
Assistant Secretary	100	0	0			
Commission on Collecting	33	0	0			
				193	0	0
Investments:—						
Purchase of £300 New 3 per Cents, 86¼	266	12	6			
Ditto £200 Consols, 89¼ ...	178	15	0			
				445	7	6
Taxes:—						
Assessed and Income	8	2	3			
Parish Rates	13	10	10			
				21	13	1
Bills:—						
Strangeways and Co., printers ...	206	3	4			
J. Rumfitt, bookbinder	2	19	8			
J. Basire, engraver	4	8	9			
Pearson, wood-engraver	1	7	0			
Turnor Fund	4	7	3			
Annual Dinner	7	6	0			
Law expenses	6	0	0			
Insurance	10	0	6			
				242	12	6
Miscellaneous items:—						
Books and parcels	4	19	1			
Postages	34	15	2			
House expenses	22	18	8			
Expenses of evening meetings ...	13	13	0			
Waiters attending meetings	3	17	0			
Coals and Gas	16	12	11			
Repairs	4	6	0			
Sundries	14	14	2			
Mrs. Jackson's annuity, 1 year ...	8	16	3			
				124	12	3
Balance at Banker's	...		...	307	17	2
				£1335	2	6

Audited and found correct, this twenty-sixth day of January, eighteen hundred and sixty-six, by us, the undersigned duly appointed Auditors of the Society,

JOHN BROWNING,
S. M. DRACH,
WILLIAM SIMMS.

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Assets and Present Property of the Society, January 1, 1866:—

			£	s.	d.	£	s.	d.
Balance at Banker's			...		...	307	17	2
1	Contribution of 7 years' standing	...	14	14	0			
1	,, 6 ,,	...	12	12	0			
2	,, 5 ,,	...	21	0	0			
4	,, 4 ,,	...	33	12	0			
4	,, 3 ,,	...	25	4	0			
40	,, 2 ,,	...	168	0	0			
40	,, 1 ,,	...	84	0	0			
2	,, various sums	...	10	15	0			
						369	17	0
Due for Publications of the Society						3	4	6
£2300 Consols (including the Lee Fund).								
£5000 New 3 Per Cents (including Mrs. Jackson's Gift).								
Unsold Publications of the Society.								
Various astronomical instruments, books, prints, &c.								
Balance of Turnor Fund (included in Treasurer's Account)						92	11	5

To the Council of the Royal Astronomical Society of London.

Gentlemen,—We, the undersigned, duly appointed Auditors of the Society's Accounts for the year ending 31st December last, have this day searchingly investigated the same, and find a cash balance at the Banker's in favour of the Society of three hundred and seven pounds seventeen shillings and twopence (£307 17*s.* 2*d.*), which includes one shilling and one penny petty cash overpaid by and due to your Assistant Secretary.

We find further that the Funded Stock now possessed by the Society amounts to five thousand pounds New Three per Cents, and two thousand three hundred pounds Consols, yielding dividends of £219 per annum.

We further beg to record our unanimous conviction that the Accounts of the Society, as presented to us, have been kept with unnecessary complexity; and we earnestly recommend a return to the simpler method formerly adopted.

As witness our hands, this twenty-sixth day of January, eighteen hundred and sixty-six.

S. M. Drach,
John Browning,
William Simms.

Royal Astronomical Society's Apartments, Somerset House, London.

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The following Revenue Account and Balance Sheet, which are made out in accordance with the plan adopted for the year 1863, show the exact position of the Society both as regards assets and liabilities:—

REVENUE ACCOUNT FOR THE YEAR 1865.

Dr.

	£	s.	d.	£	s.	d.
To Composition fund	...	...		189	0	0
Turnor fund	15	0	0			
Lee fund	3	5	6			
Mrs. Jackson	8	16	3			
				27	1	9
Rent	...	...		—	—	—
Editor	60	0	0			
Printing	145	8	6			
Engraving	8	15	9			
Binding	1	9	8			
Assistant-Secretary	100	0	0			
Collection	33	0	0			
				348	13	11
Law	6	0	0			
Taxes	21	15	11			
Postage	34	15	2			
Parcels	4	19	1			
House Expenses	22	18	8			
Coals and gas	18	8	11			
Meetings	17	10	0			
Medal	10	10	0			
Insurance	10	0	6			
Repairs	4	6	0			
Sundries	22	2	2			
				173	4	5
Sums written off	38	17	0			
To Capital—increase	500	5	11			
				539	2	11
				£1277	3	0

Cr.

	£	s.	d.
By New compositions	189	0	0
Composition fund—deaths	105	0	0
Admission fees	60	18	0
First year's contributions	34	13	0
Annual contributions	554	8	0
Carried forward	£943	19	0

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						Brought forward	£943	19	0
Dividends, January	...	...	...	£30	14	3			
,, April	...	...	...	68	14	9			
,, July	...	...	...	30	19	6			
,, October	...	...	...	73	15	0	204	3	6
Sale of publications	...	...	...	...	...		74	8	0
By Investing under par	...	...	...	...	...		54	12	6
							£1277	3	0

BALANCES, DECEMBER 31, 1865.

Dr. £	s.	d.		Cr. £	s.	d.
			Cash due to Assistant-Secretary ...	0	1	1
307	17	2	Banker's balance.			
			Proceeds of Turnor fund	92	11	5
			Proceeds of Lee fund	6	5	9
			Due to Mrs. Jackson	–	–	–
369	17	0	Annual contributions—arrears.			
			Paid in advance	6	6	0
8	8	0	Admission fees—4 due			
3	4	6	Due for publications.			
			Reserve against annual contributions due ...	65	0	0
2300	0	0	3 per Cent Consols at par.			
			,, Turnor fund	500	0	0
			,, Lee fund	110	0	0
5000	0	0	New 3 per Cents at par.			
			,, Jackson fund	300	0	0
			Estimated liabilities for work in progress	26	3	6
10	10	0	Medal in stock.			
–	–	–	Unsold publications.			
–	–	–	Books and instruments.			
			Composition fund ... £3507 0 0			
			Net capital 3386 8 11	6893	8	11
£7999	16	8		£7999	16	8

Before the other business of the Meeting was proceeded with, a discussion arose respecting the best mode of keeping the Financial Accounts and of presenting them annually to the Society, whereupon the following Resolution was duly proposed, seconded, and carried:—

"That this Society recommends that the Financial Statement be left exclusively in the hands of the Treasurer, who is to draw up the Accounts in conformity with the Bye-Laws."

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Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	16	XI.	194	XXI. (together).	101
I. Part 2	57	XII.	201	XXII.	192
II. Part 1	75	XIII.	215	XXIII.	190
II. Part 2	39	XIV.	402	XXIV.	193
III. Part 1	91	XV.	183	XXV.	208
III. Part 2	110	XVI.	207	XXVI.	214
IV. Part 1	110	XVII.	185	XXVII.	472
IV. Part 2	122	XVIII.	185	XXVIII.	432
V.	137	XIX.	198	XXIX.	459
VI.	157	XX.	190	XXX.	216
VII.	180	XXI. Part 1 (separate).	316	XXXI.	198
VIII.	167			XXXII.	230
IX.	171	XXI. Part 2 (separate).	100	XXXIII.	258
X.	183				

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock, No. 1,
The *Beaufoy* clock, No. 2,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle,
The *Brisbane* circle.

The *Sheepshanks'* collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and

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siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. 4$\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters.

4. 3$\frac{1}{4}$-inch achromatic telescope, with equatoreal stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. 2$\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. 2$\frac{1}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir.

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and 2$\frac{1}{4}$-inch glass plane artificial horizon.

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32. Plane speculum artificial horizon and stand.
33. 2½-inch circular level horizon, by Dollond.
34. Artificial horizon roof and trough.
35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.
36. A pentagraph.
37. A noddy.
38. A small Galilean telescope, with the object lens of rock-crystal.
39. Six levels, various.
40. 18-inch celestial globe.
41. Varley stand for telescope.
42. Thermometer.
43. Telescope, with the object-glass of rock crystal.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument,		No. 1, to Mr. Lassell.
Ditto	ditto	No. 2, to Mr. De La Rue.
Ditto	ditto	No. 3, to Rev. F. Howlett.
Ditto	ditto	No. 4, to Rev. C. Lowndes.
Ditto	ditto	No. 5, to Mr. Birt.
Ditto	ditto	No. 6, to Rev. J. Cape.
Ditto	ditto	No. 9, to Mr. Lockyer.
Ditto	ditto	No. 10, to Admiral Bethune.
Ditto	ditto	No. 41, to Rev. C. Pritchard.
Ditto	ditto	No. 43, to Mr. Huggins.

The 6-inch circular protractor, to Mr. Birt.

The Council cannot recollect any former occasion on which there has been better ground for congratulation to the Royal Astronomical Society than at the close of the past year. Looking backwards ten years, they find the number of the contributing members has increased by nearly thirty per cent. The attendance at the Evening Meetings has more than doubled, and the funded property of the Society, during the nine years' tenure of office by the present Treasurer, has increased by upwards of £2700 Stock. Applications for the supply of the *Monthly Notices* of our proceedings continue to be made from every quarter of the globe; and several of the numerous private Observatories scattered throughout the country are showing signs of increasing vitality by the production of fresh and valuable results.

Time, however, as upon all else, so upon us, has left its

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inevitable mark; and a Society which celebrates, as we do to-day, its forty-sixth anniversary, can scarcely hope to find many of the cherished names of its original founders still remaining on its lists. Of those eminent and energetic men who met together as the germ of the Royal Astronomical Society, on the 12th of January, 1820, *three* only now survive; but it is worthy of our most grateful observation, that in Mr. Babbage, Sir John Herschel, and Sir James South (for these are the three), we recognise names which are among the most honoured that have adorned our annals.

The Volume of *Memoirs* will shortly appear. It contains three Papers, these are, first, a Memoir by Mr. Stone, the First Assistant in the Royal Observatory, Greenwich; the title is "On the Accuracy of the Fundamental Right Ascensions of the Greenwich Seven-year Catalogue for 1860."

The second Memoir, also by Mr. Stone, is "Constant of Lunar Parallax."

The third Memoir, "On some peculiar instances of Personal Equation in Zenith Distance Observations," is by Mr. Dunkin, also of the Royal Observatory.

To these Memoirs are appended three communications from Sir Thomas Maclear, entitled—

1. "Geocentric North Polar Distances of the Moon and Moon-culminating Stars from Observations made with the Transit Circle at the Royal Observatory, Cape of Good Hope."

2. "Right Ascensions and North Polar Distances of Comet I., 1865, derived from Observations made with the $8\frac{1}{2}$ feet Equatorial."

3. "Mean Right Ascensions and North Polar Distances of Comet I., 1864, derived from Observations made at the Royal Observatory, Cape of Good Hope, by W. Mann, Esq. First Assistant."

The expense of printing these last Papers is defrayed by the Government.

Obituary.

During the past year the Society has to regret the loss by death of more than an average number of long-valued and distinguished Members, some of whom will be greatly missed, not alone by our own body, but by the whole commonwealth of Science. They are as follows:—

Honorary.

The Duke of Northumberland.

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Ordinary Members.

Dr. Burder.
S. H. Christie, Esq.
Benjamin Gompertz, Esq.
J. W. Grant, Esq.
Sir William Rowan Hamilton.
Rev. John Hind.
Sir John Lubbock, Bart.
F. W. Simms, Esq.
Vice-Admiral Smyth.

Associates.

Prof. G. P. Bond, of Cambridge, U.S.
Prof. Encke, of Berlin.

William Corbett Burder was born at Stroud, Gloucestershire, October 30th, 1822. His father was the Rev. John Burder, M.A., who survives him, and his grandfather was the Rev. George Burder, the author of *Village Sermons*. After serving five years' apprenticeship to an architect in Bristol, he left that profession, and, removing to London, studied architectural engraving under Mr. J. H. Le Keux. His success in this pursuit is shown by the engagement which Mr. Le Keux afterwards made with him for his services, as well as by the published portion of an unfinished work entitled *Architectural Antiquities of Bristol*, the illustrations of which he both drew and engraved.

As a meteorologist he was widely known from his long connexion with Mr. Glaisher's corps of observers, and by the notices which he frequently sent to the *Times* on matters of meteorological interest. He also published a few years since a little volume, the *Meteorology of Clifton*, containing the condensed results of ten years' observations at that place. A severely accurate and conscientious observer himself, he instinctively detected the want of these qualities in others, and was always displeased when he found that a scientific statement had been coloured to serve a purpose, or a phenomenon recorded with a show of precision beyond what the circumstances permitted. At the root of this feeling was, doubtless, the intense love of truth which distinguished him through life, and was indeed the dominant principle of his character. To the same cause may be traced the antipathy which he entertained to the whole race of weather prophets, a class of persons whom he disliked the more, as they tended to bring meteorology itself into disrepute among the half-informed.

In Astronomy, Mr. Burder found a source of genuine pleasure. A trivial incident shows how early the bent of his mind in this direction declared itself. A lady who kept a preparatory school at which he was a pupil when about seven years of age relates, that on one occasion he insisted that the

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play-hour, 12 o'clock, had arrived, because he had observed that at that hour the shadows of the window-bars took a particular position on the floor,—a very juvenile attempt at a meridian observation, and it is believed self suggested. He was ingenious in the construction of instruments, and devised a portable equatoreal, of which he published an account in *Recreative Science*. He also constructed a clock, the accurate performance of which surprised and pleased him. He often employed spare moments in the apparently unprofitable task of scouring the heavens with the naked eye for new comets, and it is remarkable that on two occasions his search was rewarded. The particulars of these two discoveries (made on March 28, 1854, and June 30, 1861), are related by the Hon. Mrs. Ward in *Recreative Science* for August 1861. His sight was very acute, as was also his ear. He had a good knowledge of music, and played agreeably on the flute and violin.

For the last two years of his life increasing weakness confined him almost entirely to his room, and during this period his mechanical ingenuity showed itself in a great variety of contrivances adapted to meet his special wants. One of these was a mirror, so placed as to reflect into his room the image of a vane erected on the roof of the house. Another was a ventilating apparatus, constructed so as to convey a constant supply of fresh external air, brought up to the temperature of the apartment by passing through a certain length of tube, and discharged at a point vertically over his face as he lay in bed. His condition of chest rendering him acutely sensitive both to cold and impurity of air, he set great value on this invention, and was led into an extensive correspondence with persons whose attention had been attracted by his description of it in the *Times*.

A rather sudden accession of unfavourable symptoms occurred about six weeks before his death, and from this time no prospect remained beyond a temporary rally. Yet even through this period he retained an interest in his former pursuits, and his love of nature never failed him. One evening during this time, being told of a beautiful sunset, he requested a mirror to be placed so as to reflect it, and by this means was enabled to see it as he lay in bed. His sufferings were at intervals severe, but his mental faculties remained unclouded, and his last moments were eminently tranquil and happy. He died on the 16th of October, 1865, at the age of 43, devotedly beloved by those who stood nearest to him, and sincerely lamented by a wide circle of friends.

Benjamin Gompertz, a member of the Jewish community, was descended from a family that long held a distinguished position in Holland. His grandfather on the mother's side, Benjamin Cohen by name, was on intimate terms with William

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Prince of Orange; and it is related that during the troubled times of the first French Revolution the Dutch Stadtholder found a ready asylum at the magnificent mansion erected by his friend at Amersfort. Benjamin Cohen was a man of intellectual tastes, evinced, among other ways, by his causing a translation of Euclid into Hebrew to be made for his use. The father of Benjamin Gompertz was a successful diamond-merchant, a pursuit for which the Dutch appear to possess a singular aptitude even to the present day. The literary tastes of the grandfather descended to the grandchildren, and all the brothers of Benjamin Gompertz became, like himself, men of mark in their generation. His brother Isaac wrote poems, possessing a merit sufficient to attract the approbation of Byron. Ephraim Gompertz, still living, is known among a large circle of acquaintance to be an accomplished and original mathematician. Lewis Gompertz, the youngest brother, was the founder of the "Animals' Friend Society," and he entertained and published many a strange speculation on the relation of the brute creation to man.

Benjamin Gompertz, the subject of this notice, was born on the 5th of March, 1779, at Bury Street in the City of London. He did not enjoy the advantage of a collegiate education, but upon the whole must be considered as a self-taught man, and to this circumstance, perhaps, may be attributed some of the originality of pursuit and of conception for which he was remarkable. He became familiar with the writings of the French mathematicians of the last century, but his favourite authors were Emerson and Maclaurin, and beyond all others Newton. So great an admirer was he of the latter immortal philosopher, that to the last he retained his predilection for the "Fluxional Calculus," not only with respect to the spirit of its conception, but even for the symbols of its notation. The form of his mind seems to have been eminently truthful, and hence, one among other reasons which he assigned for his rejection of the differential notation of Leibnitz was, that it was "furtive."* In this paper, Mr. Gompertz also proposes what he considered to be certain improvements in the notation of partial fluxions or differential coefficients, and in the symbols for denoting logarithms and antilogarithms; but especially one for the embodiment of zeros which may fairly be considered as a great boon. Thus, according to Mr. Gompertz, 898,000,000 or 000,000,898 may be written with great convenience $898\textcircled{6}$ and $\textcircled{6}898$ respectively.

Mr. Gompertz while still a youth, in fact as early as the year 1798, took a prominent part among the English mathematicians of the day. He was a constant contributor to the

* "I call the differential notation furtive," says he in the *Phil. Trans.* for 1862, "on, I think, a moral ground. The moral ground is, that it appears to give to Leibnitz a greater claim to originality, to the prejudice of Newton, than I think he is justly entitled to."

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ingenious questions contained in the *Gentleman's Mathematical Companion;* and the extent of his analytical powers was fully proved by the fact, that although such men as Ivory, Nicholson, Griffith, Davies, and others, were constant contributors, nevertheless, from the year 1812 to 1822, and without a single exception, Benjamin Gompertz carried off the prize annually offered for the best solution of the prize question.

Young Gompertz was originally intended by his father to follow a purely mercantile profession, and with this view he made his first start in life on the Stock Exchange, and continued a member of that body almost to the time that he undertook the management of an Insurance Office.

In 1810 he married Miss Abigail Montefiore, the sister of the well-known philanthropist, Sir Moses Montefiore. Two daughters still survive this union. The din of Change Alley was but little congenial to the mind of Benjamin Gompertz, and, like his eminent friend Francis Baily, he was only too glad when the hour came that he could retire to the quiet of his own study, or to the instructive discussions of the various learned societies of which he became himself an active member. It was here that he rejoiced to cultivate a friendly interchange of thought with such men as Sir John Herschel, Sir James South, Mr. Babbage, Mr. De Morgan, and many other well-known names in the household of science.

The first learned society that Mr. Gompertz joined was the "Mathematical," whose members met at Crispin Street, Spitalfields. The object of this society, founded so far back as 1717, was to assist the middle classes in their cultivation of mathematical pursuits, by affording access to books on scientific subjects which at that day were accessible to the opulent alone. In the operations of this valuable society, Mr. Gompertz took a prominent part, and his great abilities and constant urbanity there, gained for him so much good will, that eventually he became President of the society, which office he retained until the members amalgamated themselves with the Royal Astronomical Society, and transferred to us their valuable collection of books and instruments.

In 1806 Mr. Gompertz contributed his first original paper of importance to the Royal Society, through Dr. Maskelyne, the Astronomer Royal of that day. This memoir treats of the summation of certain series by the method of differences, which theretofore had been summed by Landen through the aid of imaginary quantities. It was possibly the composition of this memoir which induced Mr. Gompertz to direct his attention to the interpretation and calculus of imaginary symbols, which soon occupied his thoughts for a considerable period. The results of these speculations he published in a series of tracts at his own expense, and next to his mathematical expression for the law of mortality, he even considered them as the most successful of his essays.

In 1819 Mr. Gompertz was elected a Fellow of the Royal

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Society, and for forty-six years he continued an active member of that illustrious body, and occasionally served as a member of its Council.

In the year 1820 occurs the foundation of our own Society, and if Mr. Gompertz cannot be with strictness considered as one of its actual founders, it is certain that he became one of its warmest and most active supporters from the day of its foundation. On Feb. 9, 1821, he was elected on the Council, and of it he continued a most valuable member for nearly ten years. Many of the most valuable contributions to the *Memoirs* of our Society were committed to his care, and not a few thus entrusted to him were elucidated and rendered more complete by his explanatory notes. Instances of this will be found in Littrow's paper "On the Transit Instruments," (*Astron. Trans.* vol. i. p. 275), and in Kreil's paper "On the Equatorial," vol. iv. p. 501.

The first contribution made by Mr. Gompertz to our *Memoirs* was in 1822, on the subject of "The Aberration of Light;" and in this memoir occurs a passage which it may not be inopportune here to transcribe, not alone for the truth which it contains, but for the sake of the picture which it presents to us of the enthusiasm with which Gompertz was animated in his pursuit of science. "In the contemplation of the sciences," he says, "there is, besides the pleasure arising from the acquirement of knowledge of practical utility, a peculiar charm bestowed by the reasoning faculty itself in a well-directed pursuit of facts; and though the results deduced by the arguments are frequently considered to be the only objects of value by the unlearned, the man of absolute scientific ardour will often, while he is enraptured with the argument, have not the slightest interest for the object for which his argument was instituted." This remarkable evolution of Mr. Gompertz's mind may probably serve as the clue explaining the fact that, although he enriched the *Memoirs of the Royal Astronomical Society* with many admirable contributions on the subject of the corrections of astronomical instruments, he in no other sense ever became a practical astronomer, or was much habituated to the use of the instruments, with the construction and errors of which he was thus manifestly familiar. Thus we have in the first volume of our *Memoirs* (Nov.) papers and a supplement on the "Theory of Astronomical Instruments." Again, in the volume for 1824, there occurs the description of a new astronomical instrument, named by the late Francis Baily "The Differential Sextant." There is also a contribution on the subject of the corrections due to errors in the knife-edges of Kater's Convertible Pendulum; but the most practically important astronomical object to which Mr. Gompertz directed his attention was that in connexion with formulæ for the reduction of the apparent places of the stars to their mean co-ordinates. What Mr. Gompertz

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effected in this direction may probably be most easily understood from the following quotation of words spoken by Sir John Herschel in his well-known *éloge* of the late Francis Baily: "It seems almost astonishing that these computations, which lie at the root of all astronomy, and without which no result can be arrived at, and no practical observer can advance a single step, should have remained up to so late a period as the twentieth year of the nineteenth century in the loose and troublesome state which was actually the case, and that not from their theory being not understood, but from their practice not having been systematized. . . . Messrs. Baily and Gompertz, perceiving this want, proceeded to supply it. The subject was investigated generally, and a method was devised for arranging the terms of the corrections for Aberration and Solar and Lunar Nutations. Some of the tables had already been computed, when they heard of Bessel's labours in the same field. Finding that that astronomer had proceeded on a similar principle, but that besides the other corrections he had taken in that of Precession, Messrs. Baily and Gompertz willingly gave way, and certainly nothing more perfect than the 'Elementa Astronomica,' by Bessel, could have been devised. However, Messrs. Baily and Gompertz were of some use. The complete Catalogue of the Royal Astronomical Society may be partly looked upon as the fruit of the united labours of these two men."

Such, then, are the labours of our late most excellent member in that field of science which we especially cultivate. With the exception, indeed, of certain investigations, unfortunately left incomplete, on meteors and shooting-stars, which occupied his attention during a part of the last years of his life, his most recent contribution to our *Memoirs* is dated so far back as the year 1829.

It does not seem to be the province of our Society to record, more than by way of the most general reference the labours of Mr. Gompertz in that branch of science in which, perhaps, no man has ever been more highly distinguished We allude to his admirable and most valuable contributions to the theory of Life Assurances and Contingent Reversions, which never ceased to occupy his thought during the remainder of a long and valuable life. The name of Gompertz must be fresh in the recollection of every actuary who truly studies his own profession; and it may not be too much to say that his writings will ever form one of the landmarks in a science which silently but effectually intertwines itself into the material interests of society.

It is thus that, in a brief paragraph, we simply, as members of an astronomical society, pass over a long period of activity replete with interest to other men whose tastes and pursuits are attracted or determined by the interest they take in all that concerns the statistics of human life. In other and

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more fitting publications they will find the records of these labours of this remarkable man. To us it must be of interest to know that, respected by those who saw him only at a distance, or through the medium of his works, and beloved by his family and those who saw him in the closer relations of life, Benjamin Gompertz, at a ripe old age, died on July 15, 1865, and was buried in the Jewish Cemetery, near Victoria Park.

William Rowan Hamilton, one of the ablest mathematicians that this or any other country has produced, and for nearly forty years a Fellow of the Royal Astronomical Society, was born in Dominick Street, Dublin, in the year 1805. His father was by profession an attorney, and was long held in great estimation both for his personal character and his professional ability. The branch of the Hamilton family from which he was descended, originally settled in the north of Ireland, in the reign of James the First; and it is said that by right a baronetcy belonged to the representative of this branch, a near relative of his own; although the claim could not be fully supported, owing to merely technical flaws. Thus Hamilton may have been in some degree indebted for his great and versatile mental capacity to a mixture of race.

William Hamilton is one of those rare instances, where the promise of early childish precocity has not been disappointed by the attenuated achievements of riper years. At various stages of his boyhood, not to say childhood, for the precocity manifested itself at the early age of *four*, he is said to have successively acquired some notable acquaintance with no less than thirteen languages, European and Asiatic. His attention was directed to the latter, because it was originally hoped that, enjoying as he did the opportunity of good patronage, his career would be passed in India. It is recorded on evidence which deserves respect, that at the age of seven he was examined in Hebrew by a fellow of Trinity College, Dublin, and that "the child passed a better examination in that language than many candidates for the fellowship." For obvious reasons we hope there is some pardonable though very natural exaggeration in the statement. It is certain however that the attention of the Persian Ambassador, when on a visit to Dublin, was attracted by a letter of greeting written in Persian by young Hamilton at the age of fourteen. Whether or not any allowance is to be made for the shadow of the future overlapping the memory of the past, it is quite certain that the vast intellectual capacities of the boy were evinced and cultivated at a very early age, and what is of far greater consequence, this early mental activity did not prostrate or forestal the successful exertions of maturer life. It is quite possible that the literary turn thus given to his earlier pursuits may have happily laid the foundation of that peculiar combination of metaphysics and poetry, which distinguished

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some of his mathematical performances from those of most other men. For his early training in ancient and modern languages, he was indebted to the loyal care of his uncle, the Rev. James Hamilton, curate of Trim; but in science and mathematics he appears to have been nearly self-taught and self-directed; in his case, as in that of many other eminent men, this circumstance probably conduced to the originality of his maturer conceptions, and to the peculiar style in which he embodied them.

By the age of fifteen, young Hamilton had mastered the usual course of elementary mathematics, pure and applied; and in some instances had become familiar with works of original research. He appears to have evinced a peculiar taste for long and difficult arithmetical approximations, and to have shown himself no mean antagonist in the solution of numerical puzzles when matched against a certain arithmetical prodigy, who, coming from America, happened at that time to be exhibited in Dublin. By the age of seventeen, he had mastered Newton's *Principia*, and a year later found him in possession of most of the processes in the *Mécanique Céleste*. Meanwhile, and notwithstanding this very unusual advancement in mathematical knowledge, the main culture of his mind had been classical; and that, not alone from natural predilection, but on account of the requirements of the collegiate course on which it was his intention to embark and to compete.

It is almost needless to say that young Hamilton, with a mind thus disciplined and furnished, entered upon his course at Trinity College, Dublin, if not without able competitors, at all events without an equal, whether in literature or mathematics. As might be expected, he carried all before him; and when we speak of success in his literary efforts, it must be understood that we include Poetry in the list, inasmuch as on two successive occasions he gained the Vice-Chancellor's Prize for English verse. It is to this early and successful cultivation of the lighter elegancies of scholarship that his friends were indebted for a vein of poetical thought and expression which graced alike his correspondence and his conversation, and which is sometimes observable even in his graver compositions.

It appears that in the year 1822, one year before his entrance at the University, young Hamilton, now in his eighteenth year, attracted the notice of the celebrated Dr. Brinkley by certain objections which he made to a demonstration propounded by Laplace in the *Mécanique Céleste*. On being invited to pay a visit to that well-known astronomer, the young student thought that he should most properly express his feelings of respect by carrying in his hands another instance of independent research on the osculation of certain curves of double curvature. This introduction of Hamilton to the veteran professor laid the foundation of a mutual friendship

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and respect which continued to increase during Dr. Brinkley's tenure of office.

In the first year of his student life at Dublin, Hamilton, notwithstanding his close attention to the elementary line of study necessarily prescribed to under-graduates, nevertheless engaged himself in a line of original research. Even before his entrance at the University he had directed his thoughts to the difficult subject of Caustics, and having now completed the memoir, it was read before the Royal Irish Academy in 1824. This paper was referred as usual to the consideration of a committee of scientific men, who, being struck with the originality of the conception, and the evidences of analytical power which it contained, recommended the author to give those further developments of the subject which evidently lay within his grasp. The result of this encouragement to the young philosopher was the speedy completion of a memoir which may be said to contain the germ of a large portion of the noble work which it was his lot to contribute towards the advancement of physical knowledge. Instead of an essay on Caustics, his paper was now enlarged into a wider and more general investigation, under the title of a "Theory of Systems of Rays." It may be no exaggeration to say of this memoir, in conjunction with its subsequent supplements, that it is one of the ablest contributions ever made to our knowledge of the geometry of optics. Chasles, one of the most distinguished of modern geometers, speaks of it as "*dominant toute cette vaste théorie.*" Starting from the simple fundamental principle that light, whatever may be its cause or its constitution, is amenable to what mathematicians call "The Principle of Least Action," or, in other words, probably as true, and certainly more expressive, amenable to the *principle of no waste in nature*, Hamilton, in a train of analytical logic unimpeachable, and with a mastery over the management of algebraic symbols probably never surpassed, shows that the theory of a system of rays reflected or refracted any number of times at given surfaces, depends on the determination of a single principal function V, which contains in itself all the properties of the system of rays, in a manner analogous to that in which the properties of a curve are contained in its equation. The same theory is, in the supplements, extended to the more complicated and recondite question of double refraction in biaxal crystals, and at length lands the reader in one of the most remarkable scientific predictions contained in the records of physical inquiry. But of this prediction we must speak presently.

When the first part of this "Theory of Systems of Rays" was presented in April 1827 to the Royal Irish Academy, it will be remembered that Hamilton was as yet an undergraduate of twenty-one years of age. In this year the Professorship of Astronomy in Trinity College, Dublin, became vacant by the promotion of Dr. Brinkley to the bishopric of Cloyne. Such

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was his deserved reputation, that, notwithstanding the appearance of other and most formidable candidates in the field, and although, moreover, he had as yet taken no academical degree, Hamilton was elected to the vacant chair.

This circumstance is of itself sufficiently remarkable, and reflects equal honour upon the authorities who ventured to make the appointment, and on the young geometer who, by dint of genius and laborious study, was qualified to discharge the duties of the post. In connexion with this arrangement there is a point of osculation with our own Society of sufficient interest to demand our notice. The present Astronomer Royal, at that time Lucasian Professor of Mathematics in the University of Cambridge, was one of the candidates for the vacant post at Dublin; and he too, like Hamilton, had been advanced to his professorship before he had ceased nominally to be in pupilage. We are not here, even by the remotest implication, suggesting a comparison between these eminent men; such a comparison would not only be utterly unfitting, but, owing to the divergence of the lines of research adopted by the two Geometers, would be wholly impossible. Nevertheless the thought unavoidably presents itself, that for both parties, and for the general interests of science, the decision of the Dublin electors was a happy one. Had it been otherwise, the one, in all probability, from certain natural tendencies of his mind, would have become a clergyman—no doubt a most eminent one—in the Irish Church; while Greenwich and our own Society might have lost the other.

> "There is a Divinity which shapes our ends,
> Rough-hew them how we will."

In 1828 Hamilton became a Fellow of the Royal Astronomical Society, and thus at the time of his decease was among the oldest, as his name was certainly among the most honoured, of our members. In 1833 he made known, in one of several supplements to the "Theory of Systems of Rays," his great discovery of Conical Refraction. In this memoir, starting again from the principle of least action, and, as before, conducting the investigation by means of a single Principal Function, he establishes the entire theory of double refraction; and, applying it to the case of biaxal crystals, by a new and simpler method* than that originally pursued by Fresnel, he obtains the equation to the form of the wave assumed by the vibrating ether within the crystal. On examining the form of the wave surface, Hamilton, with remarkable sagacity, observed that if the theory and the results were true, a single ray of light incident at a certain angle on a biaxal crystal, must of necessity pass into it, not as one ray,

* It is but a point of justice to state that Mr. Archibald Smith has subsequently much improved the simplicity of the process by a very elegant method of elimination.

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nor even as two rays, but as a conical sheet of light, and then finally emerge as a luminous cylindrical surface. And, again, his profound and complicated analysis indicated that there was also a direction within the crystal, such, that if an internal ray of light passed along it, it would emerge from the crystal, not as one ray, but as a luminous conical shell. Such results as these were not only apparently contrary to all analogy and expectation, but formed, if the experiment could indeed be made, a species of *experimentum crucis* of the truth of the undulatory theory of light. Notwithstanding the difficulty of the case, the experiment was at length successfully performed by Dr. Humphrey Lloyd, of Dublin, whose patient ingenuity, and faith in the profound work of the geometer, were rewarded by the sight, for the first time, of what cannot properly be called less than the astonishing phenomenon of a single ray spread out, by refraction in a crystal, into an infinite number of rays, forming the surface of a luminous cone.

From the sagacity of Hamilton, and of his friend Dr. Lloyd, thus constraining the little crystal of Arragonite to give up, Sphinx-like, its secret of ages, our thoughts unavoidably turn to the parallel case of Adams and Leverrier, who, from a similar strong faith in the laws of nature and in the logic of geometry, not only predicted the existence of a planet heretofore unseen and unexpected, but indicated the precise region of the heavens, where, as soon as looked for, it was actually found. We do not regard such results as valuable only because they corroborate our conviction of the existence of certain laws whereon we believe the universe to have been constructed by the Author of Nature, but still more so because they serve to encourage the student to persevere in his researches, animated by the fullest conviction that if truthfully conducted they can only land him in truth, and leaving the *cui bono* to be determined by the appreciations, or the wants, or the curiosities, of men in time to come.

The Royal Irish Academy took cognisance of Hamilton's great discovery, and of the profound mathematical skill whereby it was evolved, by conferring upon him their Cuninghame medal; and the Royal Society awarded to him a similar mark of their appreciation of his merits. In 1837 he was elected President of the Royal Irish Academy, succeeding* his friend and early patron, Dr. Brinkley, in the chair, as he had succeeded him in the Professorship of Astronomy. He retained this distinguished office for eight years, and on his resignation he received the thanks of that eminent Academy "for his high and impartial bearing in the chair."

In 1834 and 1835 he communicated to the Royal Society two papers on "A General Method in Dynamics." Here,

* Dr. Lloyd, sen., was President for two years after the death of the Bishop of Cloyne. Hamilton succeeded Lloyd.

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again, he commenced with the same fundamental idea, as that which he had already so successfully adopted in his "Theory of Systems of Rays," and he showed that the integration of the differential equations of motion for any system of bodies may be considered as depending on the determination of a certain Principal Function, which he defines in several different forms, but in each case by means of *two* partial differential equations involving, one of them, the differential coefficients in regard to the final co-ordinates (co-ordinates at the time t), the other, those in regard to the initial co-ordinates of the several particles. He also established in these Memoirs the now well-known "*Hamiltonian Form*" of the equations of motion of any material system.

The two Memoirs just referred to gave occasion to Jacobi's investigations on "Partial Differential Equations" (*Crelle*, t. xvii. 1837). Jacobi shows that, instead of "Hamilton's Function" involving the time and the initial and final co-ordinates, and satisfying *two* partial differential equations, it is allowable to consider a function of the time and the final co-ordinates only, satisfying a *single* partial differential equation; and he considers that by omitting to make this simplification, Hamilton presented his remarkable discovery in at least an imperfect light. There can be no doubt that the simplification thus introduced by Jacobi was a most important and valuable one; but it can scarcely be objected to Hamilton that he failed to perceive all the results deducible from his own discovery, any more than it can be objected to Fresnel that he left it to Hamilton to deduce conical refraction from the very form of the wave surface which Fresnel was the first to investigate. It must not be forgotten that it is to Hamilton's discovery as their fountain, though the course of the stream was directed by Jacobi, that are due all the developments which have since been made in the vast subject of Theoretical Dynamics. In a word, it may not be too much to say that the step in advance made by Hamilton's two memoirs can only be compared with that effected at an earlier epoch by the publication of Lagrange's *Mécanique Analytique*. For this work, also, Hamilton was again awarded a gold medal by the Royal Society.

We pass over various other characteristic works of this profound analyst, not because they are devoid of interest or of worth, but because they are less within the scope of our Society; and we come at length to what Hamilton considered the crowning labour of his life,—a labour which for the next twenty years, and indeed till within a few days of his decease, continued to occupy his thoughts. The labour here referred to was bestowed on the invention and the development of the Calculus of Quaternions. In a memoir such as this, and for the purposes which we have in view, we must almost despair of explaining, or perhaps of even conveying an

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idea of what is the aim and scope of the Calculus of Quaternions, or in fact what a Quaternion is, and yet without some such attempt, successful or not, any obituary notice of this great man would be incomplete. For this purpose, then, we must bear in mind that in the method of geometry introduced by Descartes, and which has been retained in astronomical and physical investigations up to the present time, the position of a point in space has been determined either by its distances from three co-ordinate planes, or by what in reality are their equivalents. Hamilton, however, starts at once by considering not so much the position of a *point*, as rather the relation which exists between two *lines* intersecting in space, having regard both to length and to position. It will soon be seen that in order to determine these relations completely, four quantities, or four elements, are necessarily involved.

1°. There is the relation which the length of the one line bears to the length of the other line;

2°. The angle through which the one line must be conceived to be turned in order that it may coincide with the direction of the other;

3°. The plane in which the two lines lie.

And inasmuch as the determination of this plane involves two elements, viz., 1°. its inclination to some fixed or known plane, and 2°. an element which is analogous to the longitude of a planet's node, it follows that four* elements or symbols are required to determine the relation which one line in space bears to another line.† The combination of these four elements, then, forms the Quaternion of Sir William Hamilton; and as handled and developed by him, these combinations unquestionably form a calculus of amazing generality, grasp, and power. As an engine of investigation, in the general problem of

* The above is in fact one of Hamilton's many illustrations of the meaning of a quaternion. Analytically speaking, a quaternion is an expression of the form $w + ix + jy + kz$, where i, j, k are imaginary roots of $\sqrt{-1}$, differing from the imaginaries of ordinary algebra, in that the *order* of multiplication of these symbols is material, ij here not being $= ji$ but $= -ji$, and so for the other symbols.

The geometrical interpretation is this: on taking the usual three rectangular co-ordinate axes of x, y, z; if ij means rotate the axis of (y) round the axis of (x) through 90° of right-handed rotation, then ji must mean rotate the axis of (x) round the axis of (y) through 90° of right-handed rotation. Now the result of the former rotation is a line in the direction of the axis of $+z$; the result of the latter rotation is a line in the direction of the axis of $-z$: in this sense then $ij = -ji$ and so $jk = -kj$, and $ik = -ki$. The symbol (w) is the ratio of the lengths of two intersecting lines (or *vectors*) considered in the quaternion. Such is a first glimpse of this intricate Calculus.

† *Elements of Quaternions*, Longmans, 1866, page 110. This extraordinary work is the result of the unceasing labours of the two last years of Sir William Hamilton's life: indeed, it is said to have been fatally injurious to his health. It was all but finished when the lamented death of the author arrested its entire completion. The Board of Trinity College, Dublin, have marked their sense of the value of this book by defraying the expenses of its publication.

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combined rotations, the method of Quaternions probably has no rival in completeness or in facility. They remind one of the tentacles of some gigantic polype, ramifying out into immensity, and bringing back with them the spoils of space.*

It is as yet premature to anticipate on which of his investigations or discoveries Hamilton's fame will ultimately rest. There are mathematicians among us who in this respect would be inclined to name his Calculus of Quaternions; others would say that none of his writings can overshadow the importance of his *Dynamical Theorems*. As yet, however, the former Calculus can hardly be said to be fully developed, or to have been extensively applied by other philosophers to new lines of investigation; nevertheless, it can scarcely be supposed that the persistent and conscientious labour of such a man for twenty-two successive years can fail to be full of the seeds of thought, and one day be found to admit and to invite important applications. It must however be conceded that (partly perhaps on account of its comparative novelty, and partly on account of the metaphysical atmosphere which surrounds it), the method is neither easy nor attractive to any but the ablest and most daring of the analysts among us; many a man who has essayed to bend this bow has probably said to himself what Antinous said to his boon companions:—

> "Thou wast not born to bend
> The unpliant bow, or to direct the shaft."†

We have just spoken of the metaphysical atmosphere which seems to pervade Hamilton's Calculus of Quaternions; and herein there is little to excite our surprise, for it was natural for a man possessed of a mind so versatile and so profound, to turn it inwards on itself; hence he delighted in metaphysics. But it was not alone because the culture and bias of his mind unavoidably led him in this direction, that many of his mathematical investigations assumed a metaphysical turn, but because he, in conjunction with other thoughtful philosophers, believed that no further great advance in mathematical science was now to be expected, excepting from the metaphysical point of view. Probably it is either a conscious conviction, or an intuitive perception of this, which influences the peculiar phase observable in the mathematical investigations of some of our greatest analysts of the present day.

Hamilton was not only a great mathematician, but by nature he was also a poet. He was heard to say, "I *live* by mathematics, but I *am* a poet." If, by this aphorism, he meant that, had he so chosen, he would have become more eminent as a poet than he is as an analyst, bystanders might hesitate to give their assent. Few men, perhaps, are fully

* With this simile Sir W. Hamilton expressed his acquiescence to the writer of this memoir.

† Cowper's translation of the *Odyssey*, book xxi.

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conscious of the ruling bias and the strong points of their own minds. We know one of our greatest living philosophers who would perhaps say, "By filial duty I am an astronomer, but I was born a chemist." Of another it has been often said, "He *is* a mathematician and an observer, but he *was born* an engineer." Nevertheless Hamilton was a true poet, and by no means an indifferent writer of true poetry; and it is quite certain, that some of our subtlest mathematicians are poets at heart, knowing it and feeling it. And here it may be worth a passing remark to mention that Hamilton, in his great Memoir on *A General Method in Dynamics*, speaks of Lagrange's *Mécanique Analytique*, as a *Poem*. One of our chief living astronomers hereon remarks: "Hamilton was right, but he might have said a poem of most stately rhythm." The two works of Lagrange and Hamilton have points in common.

Hamilton counted among his friends, Coleridge, Southey, Mrs. Hemans, and Wordsworth. It is said, that when the latter, through Hamilton's enthusiasm, was enabled to get a glimpse of the inexpressible fascination which surrounds the daring and *creative* spirit of modern geometry, the old man was for the first time inclined to admit even a mathematician into the charmed circle of the brotherhood of poets. The anecdote rests upon unquestionable authority: nevertheless, we are inclined to think better things of so great and profound a mind as that of Wordsworth, and we are convinced that he must, by sheer dint of sympathy with other minds, have had at least a suspicion of the fact before the great analyst revealed it. In vindication of the justness of these remarks on the expansiveness of great intellects, and on the poetic power which almost invariably is, at the least, latent within them, we cannot refrain from quoting the following Sonnet, written by a great Astronomer, on the occasion of a visit to Ely Cathedral, in company with Sir William Hamilton:—

Sunday, July 29, 1845.

The organ's swell was hushed,—but soft and low
An echo more than music rang,—where he,
The doubly-gifted, poured forth whisp'ringly,
High-wrought and rich, his heart's exuberant flow,
Beneath that vast and vaulted canopy.
Plunging anon into the fathomless sea
Of thought, he dived where rarer treasures grow,
Gems of an unsunned warmth, and deeper glow.

Oh! born for either sphere, whose soul can thrill
With all that Pöesy has soft or bright,
Or wield the sceptre of the sage at will,
(That mighty mace * which bursts its way to light),
Soar as thou wilt, or plunge,—thy ardent mind
Darts on—but cannot leave our love behind.

* The symbolic analysis of which the eminent and excellent individual (Sir W. R. H.) supposed to be addressed has proved himself a most consummate master.—(*Essays by Sir John Herschel.*)

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This Memoir would be incomplete if we did not add, that our deceased member, together with the character of a scholar, a poet, a metaphysician, and a great analyst, combined that of a kind-hearted, simple-minded Christian gentleman; we say the latter because Sir William Hamilton was too sincere a man ever to disguise, though too diffident to obtrude, his profound conviction of the truth of revealed religion. Endued with such qualities as these, what wonder, if of his friends he was almost the idol, and of his university the pride; for he was gentle, and he was eloquent, and he spoke evil of no man, he defended the fair fame of the absent, and he held controversy with none.

Such, then, is an imperfect but unexaggerated sketch of this remarkable man. We will only add, that happily he did not live to survive himself, but in full possession of his faculties, almost in the very presence of the friends who had long admired him; and, what was no new thing to him, supported by the convictions and consolations of his faith, he resigned himself to his rest, as one who knew that he had done a work which had been given him to do. [C. P.]*

The late Sir JOHN WILLIAM LUBBOCK, Bart., born in 1803, was educated at Eton, and at Trinity College, Cambridge, where he graduated in 1825, being first in the Senior Optimes.

He was many years Treasurer and Vice-president of the Royal Society, and Vice-chancellor of the University of London; was a member of most of the principal scientific societies in this country, of the American Academy of Arts and Sciences, and of the Academies of Turin and Palermo. He spent much labour upon the theory of the Moon, referred to in Grant's *History of Astronomy*, pp. 120–162. He also did much to improve the theory of the tides, for which he received one of the royal medals in 1833. The tides in the *Nautical Almanac* were for some time calculated from his tables (*Nautical Almanac*, 1848, part xi.).

In conjunction with Mr. Drinkwater Bethune he wrote an essay *On Probability* for the Society for the Diffusion of Useful Knowledge. This treatise, when reprinted, owing to some mistake of the binder, was described as "De Morgan on Probabilities." This extraordinary mistake was not discovered by Sir J. Lubbock until the work had been in circulation for some years; and Prof. De Morgan, in a letter to the *Times*, stated that "he could not, in fifteen years, though using every opportunity, succeed in restoring the book to its true authors."

* In the preparation of this *éloge*, the writer has received much assistance from Dean Graves, P.R.I.A.; the Rev. R. P. Graves, of Dublin; and Professors De Morgan and Cayley.

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There can be no doubt that among all the mathematicians who have written upon the Lunar Theory, confessedly among the most difficult and most important questions in Physical Astronomy, Sir John Lubbock has a claim to a most conspicuous place. What he says of himself and of his coadjutors, in his Memoir published in the *Transactions of the Royal Astronomical Society* for 1860, is fully within the limits of justice and truth: "I am confident that a just posterity will give to us—that is, to Plana, Pontécoulant, and Lubbock, who in 1846 furnished the means of constructing tables of the Moon without any empirical hypothesis—the credit of first bringing the Lunar Tables within the limits of error of observation, and thereby of bringing to perfection the solution of the problem of finding the longitude at sea by means of lunar observations." Great, very great as is the merit of Sir John Lubbock in having thus brought the *theory* to perfection, it is much to be regretted that he did not take one step more, and that the comparatively easy step, of causing the Tables to be constructed.

Sir John Lubbock was one of the Treasurers of the Great Exhibition of 1851, and was a member of several commissions, among which we may particularly mention that for determining the standards, and also that appointed to investigate the question of weights and measures. He was for many years senior partner in his bank; and his last publication of importance was one on the *Clearing of the London Bankers*, to which he appended, as a motto, the well-known passage commencing,

> "Atque equidem, extremo nî jam sub fine laborum
> Vela traham, et terris festinem advertere proram."

He resided principally at his seat in Kent, where he took the greatest possible interest in the education of the neighbouring poor. He also devoted much time to agricultural pursuits, and was a successful breeder of stock, his southdowns and shorthorns having carried off many prizes. He married Harriett, daughter of Col. Hotham, by whom he leaves eleven children.

The accompanying list contains the titles of Sir J. W. Lubbock's principal publications:—

On the Theory of the Moon, and on the Perturbations of the Planets. Eight parts.

Account of the *Traité sur le Flux et Reflux de la Mer*, of Daniel Bernouilli.

An Elementary Treatise on the Computation of Eclipses and Occultations.

Remarks on the Classification of the Different Branches of Human Knowledge.

An Elementary Treatise on the Tides.

On the Heat of Vapours, and on Astronomical Refraction.

On Currency.

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On the Determination of the Numerical Values of the Co-efficients in any Series consisting of Cosines of Multiples of a Variable Angle.

On Shooting-stars.

On the Attraction of Ellipsoids.

On Cask Guaging.

Note on the Calculation of the Distance of a Comet from the Earth.

On the Limits upon the Earth's Surface within which an Occultation of a Star or Planet by the Moon is Visible.

On the Divergence of the Numerical Co-efficients of certain Inequalities of Longitude in the Lunar Theory.

On the Census.

On some Elementary Applications of Abel's Theorem.

On the Double Achromatic Object-glass.

On some Problems in Analytical Geometry.

On a Property of the Conic Sections.

On a Property of the Conic Sections.

On the Wave Surface in the Theory of Double Refraction.

On the Variation of the Arbitrary Constants in Mechanical Problems.

On the Arabic Names of the Stars.

On the Stability of the Solar System.

On the Proceedings of the Excise Commissioners, with documents relating thereto.

Table of the Sines and Tangents (natural) for each degree of the Quadrant.

Table of the Logarithms of the Sines and Tangents for each degree of the Quadrant.

On the General Solution of Algebraical Equations.

On the Vapours of Ether, Alcohol, Petroleum, and Oil of Turpentine.

On the Conditions of the Atmosphere, and on the Calculation of Heights by the Barometer.

On Astronomical Refractions.

On the Gnomonic Projection of the Sphere.

Also, the Stars, in Six Maps.

In 1860 a most valuable paper, "On the Lunar Theory," in the *Memoirs of the Royal Astronomical Society*.

Frederick Walter Simms was born in London on the 24th December, 1803. Being in early youth of very delicate constitution, some difficulty was experienced in providing him with suitable employment, until, through the influence of his brother the late Mr. William Simms with Colonel Colby, he was despatched to Ireland as a civil assistant upon the Ordnance Survey. Active employment in the pure air of the mountains soon told favourably upon his health, and he returned to England with a tolerably vigorous frame.

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Shortly after leaving Ireland Mr. F. W. Simms became an astronomical assistant at the Royal Observatory under Mr. Pond. In the year 1835 he resigned his situation, and returned to his former occupation as a surveyor and civil engineer. He visited France with Mr. Claridge, and joined with him in the introduction to this country of asphalte for pavements. In 1836 he engaged with the South-Eastern Railway Company as a resident engineer, a considerable portion of their works being under his direction. He constructed both the Bletchingley and the Saltwood Tunnels.

In 1846 the East India Company, having resolved upon the construction of railways in their territories, made a proposal to Mr. Simms for him to proceed to India as their consulting engineer, which, after a short delay, was accepted; but his health giving way from exposure to the climate, he obtained leave of absence for some time, and visited the Mauritius. Upon the reinstatement of his health he returned to his duty, and, amongst other work, he superintended a complete survey of Calcutta, which was executed principally by native assistants.

Having completed his engagements with the Company, Mr. Simms returned to England in the autumn of 1851, with a very shattered constitution, and although careful medical attention restored him to tolerable health, he lived in retirement afterwards, occupying himself partly by the education of his son. His death took place in Torrington Square, on the 27th Feb. 1865.

Mr. F. W. Simms was the author of several useful works in his profession, the principal being a *Treatise upon the Instruments employed in Surveying and Astronomy*; a *Treatise upon Levelling*; *Practical Tunnelling*. He also edited *The Public Works of Great Britain*.

Mr. Simms was a member of the Institute of Civil Engineers, and a Fellow of the Geological Society.

Admiral William Henry Smyth was born at Westminster January 21st, 1788. He was the only son of Joseph Brewer Palmer Smyth, Esq., of New Jersey, and Georgina Caroline, granddaughter of the Rev. M. Pilkington. He was a direct descendant of the celebrated Captain John Smith, who has been termed the "Saviour of Virginia." During the American War of Independence Mr. J. B. P. Smyth warmly espoused the cause of the mother country; but the success of the revolutionists was fatal to his fortunes, and the only compensation his family ever received for the loss of large possessions, was the granting of a small annuity to Mrs. Smyth after the death of her husband, which followed closely on the termination of the war. At an unusually early age young Smyth followed the bent of his ardent predilections for a maritime life by shipping on board a West India merchantman, which for-

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tunately was commanded by an intelligent Master in the Royal Navy, who took some pains in teaching his *protégé* the rudiments of seamanship and navigation; and it may safely be asserted that he had an apt and ready pupil. He afterwards served in the East India Company's ship Cornwallis; and this vessel being purchased by his Majesty's Government and commissioned as a frigate under the command of Captain (afterwards Admiral) Johnston, his long-cherished wish to serve in the navy was at length gratified. He continued to serve under the same excellent officer in that vessel, and subsequently in the Powerful, of 74 guns, until she was paid off in 1809, during which period he saw much active service in the Eastern Seas, notwithstanding the fact that the ship, during a large portion of the time, was so crazy as to be hardly seaworthy. Mr. Smyth then joined the Milford, of 74 guns, and again became engaged in active service on the French coast; and his vigilance, activity, and courage won the regard and approbation of his superior officers. The Milford was sent to Cadiz in the autumn of 1810, and immediately on arrival, Mr. Smyth was appointed to the command of a large Spanish gunboat (the Mors-aut-Gloria), manned by a British crew of thirty-five men, and mounting a long brass 36-pounder and a 6-inch howitzer In this vessel he took an active part in nearly every operation of the flotilla, operating on the Spanish Coast. The Mors-aut-Gloria was one of a numerous fleet of gun-boats, which, from the dash and energy displayed in handling them, were called "fire-eaters," and among them the Mors-aut-Gloria was especially distinguished. On one occasion, we are told, she fired upwards of seventy rounds, and seemed to attract the particular attention of the French gunners; probably from her superior size, and the conspicuous death's-head and cross-bones with which her bows were decorated, so that their ricochet shot were constantly splashing the spray over her, and cut several of her sweeps; yet, strange to say, she sustained no damage. Notwithstanding the ceaseless vigilance required from Mr. Smyth during these stirring times, he managed to find time for reading and self-improvement; and, indeed, every moment that he could spare from his professional duties was devoted to hard study. Thus early he laid the foundation of his future reputation.

It appears that, on his arrival on the Spanish coast, Mr. Smyth at once embraced every opportunity that presented to make himself acquainted with the hydrography of the district, and the knowledge he thus acquired proved of great service in the prolonged naval operations of the war. Some excellent charts which he constructed of La Isla-de-Leon and the neighbouring coast, showing accurately the strength of the various French and Spanish batteries, having been submitted to Lord Melville, combined with his own distinguished services as an officer, procured him a lieutenant's commission, bearing

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date March 15th, 1813, which was accompanied by a note of a very gratifying character from that nobleman.

Almost immediately after the receipt of his commission, he was appointed to a command in the Anglo-Sicilian fleet at Messina under Brigadier Sir Robert Hall; and here we will take the liberty of making an extract from Marshall's *Naval Biography:*—

"One of the first services in which Lieutenant Smyth appears to have been employed was a confidential mission to the Court of Naples, then just wavering in its allegiance to Napoleon Buonaparte. Early in 1814 he proceeded to Palermo in command of the Scylla brig, having Sir Robert Hall's flag on board; and while there, was exposed to a serious personal danger. In the night of the 19th of February, being on shore with the Brigadier, he received a report that the Scylla was in flames. The wind then blew a furious gale, with heavy torrents of rain, and he had the utmost difficulty in getting a boat launched from Porta-Felice. On rowing a little way out, he perceived a large ship in flames and adrift, and that his own vessel was riding in safety."

In April 1814 the abdication of Napoleon put an end for a time to the European war, and Lieutenant Smyth immediately applied himself to a minute survey of the Island of Sicily, of which he executed numerous plans and charts, which were highly approved by Admiral Penrose, who had been appointed to the command of the Mediterranean Station, and were warmly commended by him to the notice of the Board of Admiralty. He also, at his own cost and without official instructions, carried on an extensive series of hydrographical operations connecting Sicily with the adjacent coasts of Italy and with Barbary. These voluntary labours fortunately were appreciated by the authorities at home, who, on September 18th, 1815, advanced him to the rank of Commander, and directed his charts to be engraved in the Admiralty office. It was also arranged that Captain Smyth should publish a full description of Sicily and the neighbouring islands, the Admiralty agreeing to purchase 100 copies. This work was published in 1824 in a quarto volume with numerous illustrations, and from the exhaustive manner in which the subject was treated, and the vast amount of useful information brought together, it attracted much attention both at home and abroad.

He next became engaged, in company with Colonel Hanmer Warrington, in the collection of numerous specimens of ancient architecture from the ruins of Leptis Magna in Barbary, which had been offered to the British King by the Bashaw of Tripoli. These were afterwards deposited in the Court-yard of the British Museum, but were subsequently removed to Windsor, and have served as models from which many architectural decorations have been copied.

From the close of the War to the year 1825 he was almost

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uninterruptedly employed in his Surveys of the Mediterranean; a labour, the importance of which has been fully recognised.

It is particularly interesting to Members of the Royal Astronomical Society to observe that it was during the Survey of the Mediterranean that Captain Smyth confirmed that intense love of Astronomy which he retained through life, and to the advancement of which he afterwards devoted many of the best years of his life.

Here is his own account of the method pursued in obtaining the latitudes of the principal points in the Survey:—

"Many of the principal latitudes were taken on shore with the 9-inch quintant and artificial horizon, and with the reflecting circle and sextant; but some of the first-class were obtained with a fine 15-inch altitude-and-azimuth circle, by means of sets, with the face of the instrument alternately turned to the east and the west."

During the prosecution of this survey he formed an intimate acquaintance with the great Italian astronomer Piazzi, for whose talents and character he entertained a high admiration.

In the year 1815 he married Annarella, the only daughter of T. Warington, Esq., of Naples, a lady of great ability and rare accomplishments; and who through all his scientific labours of every description was his devoted companion and assistant.

Released at length by the publication of his *Mediterranean Sea*, he resolved to cultivate the taste for astronomy which he had imbibed, and which, he tells us, "received its sharpest spur at the close of 1813, when I accidentally assisted Piazzi in reading some of the proof-sheets of the Palermo Catalogue." His first intention was to form a standard catalogue of the principal stars in the northern hemisphere, by comparison with the standard Greenwich stars, but fortunately for astronomy he afterwards abandoned the idea of "grinding the meridian." Acquiring a powerful refracting telescope in 1830, he resolved to enter "upon a wider scrutiny of the general sidereal phenomena." Settling at Bedford he matured his plans, and erected the Observatory which has since been so fully described in the publications of the Society, as well as in the work on astronomy which he afterwards published, that a detailed account of it is here unnecessary, but at the time of its erection, and for some years afterwards, it was undoubtedly the most complete and practically useful private Observatory in existence, and has been the model from which numerous other private Observatories have been built.

The principal instrument was an equatorially-mounted refractor of 5·9-inches aperture and 104·5-inches focus, the optical portion of which was considered to be the *chef d'œuvre* of Mr. Tulley; the other instruments were a beautiful 30-inch circle belonging to the Society, and an excellent transit and

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clock. It is worthy of remark that the Equatoreal was the first, or at any rate one of the first, in this country to which a clock-work driver was applied, the apparatus being devised by the Rev. Richard Sheepshanks, and by him presented to Captain Smyth. Having completed the erection and furniture of his "Uranian Temple," as he was wont to term it, he devoted himself, with an energy which has rarely been excelled, to the micrometrical measurement of the positions and distances of a numerous list of double and multiple stars, including most of an ascertained or suspected binary character, and also to physical observations of a selected list of clusters and nebulæ, from the catalogues of Messier and the two Herschels. The objects observed were as follows:—

Nebulæ	98	Binary Stars	20
Clusters	72	Triple Stars	46
Stars and *Comites* ..	161	Quadruple Stars ..	13
Double Stars ..	419	Multiple Stars ..	21

The more interesting binary stars were observed in some instances annually, and others were scrutinised at wider intervals. The whole of this work presents an instance of constant and sustained labour for a definite and distinct purpose during twelve years such as has not often been excelled in the annals of extra-meridional astronomy.

The object for which the Bedford Observatory was designed having been completed, it was dismantled in 1839; the Equatoreal was purchased by Dr. Lee, and re-erected at Hartwell House in a remarkably well-appointed Observatory designed by Captain Smyth. In making the transfer to the Doctor, Captain Smyth stipulated for the occasional use of his old favourite, which was readily conceded; and during a period of twenty years he continued to make extensive observations with it, especially after taking up his residence at St. John's Lodge, within a short distance of Hartwell.

In 1839 Captain Smyth removed to Cardiff, in order to superintend the construction of the immense floating-docks at that port by the Marquis of Bute, but he employed all his leisure hours in arranging and reducing his Bedford observations for the press. These were all embodied in the work now so familiar to the amateur astronomer, which appeared in 1844, entitled *A Cycle of Celestial Objects*, a work justly esteemed as one of the most captivating and popular treatises on elementary and especially sidereal astronomy in our language. The second volume of this work forms the "Bedford Catalogue," of 850 celestial objects arranged in order of right ascension, and embodying the results of all his micrometrical and other observations. The descriptions of the various objects are enlivened with a vast amount of general classic and antiquarian lore, introduced in a most genial spirit. The astronomical value

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of the Catalogue is of course mainly dependent on the accuracy of the micrometrical measures. This has been severely tested by many observers of experience; and it may safely be said that in this all-important element the *Cycle* will stand the most critical tests that can be applied. On the appearance of the *Cycle of Celestial Objects* the Society marked their appreciation of its value by awarding their gold medal to its author. In presenting the medal Mr. Airy, the President for the year, used these emphatic words:—

"My confidence in the exactness of the observations is purely personal. Knowing the attention which has been given to the instrumental adjustments, the intentness of the observer upon his work, the nerve, which is made steady rather than disturbed by the anxiety to procure a good observation, and the general skill in the management of the instruments, I can truly say that if an accurate observation were required I should desire that it should be made by Captain Smyth." In handing over the medal he added, "And I beg leave to convey with it the expression of my own opinion that never was a medal more worthily earned." It is twenty years and more since these words were uttered, but every observer of double stars, every amateur astronomer, will heartily endorse these sentiments.

The publication of the *Cycle* by no means terminated the astronomical labours of Captain Smyth. For many years, when leisure permitted, he returned to his telescope with all his old zeal and accuracy; and his observations, entirely on objects in the "Bedford Catalogue," were afterwards partly published in a quarto volume, entitled *Ædes Hartwellianæ*, but afterwards in a more complete form in a work entitled *Speculum Hartwellianum*, which appeared in 1860, gracefully dedicated to those who had in any way assisted him in his researches. This work is a supplement to the "Bedford Catalogue," containing the results of observations made at Hartwell between the years 1839 and 1859, bringing up to the latter date the history of many of the more remarkable double stars. Among them *γ Virginis* occupies a conspicuous place. Captain Smyth fortunately observed the perihelion passage of the comparison star in 1836, and with such rapidity was it performed that, although its period is probably somewhere about 180 years, he has actually measured it through a course of position angle of 270°! From Captain Smyth's measures alone (but we must now term him Admiral, for he was gradually promoted to the highest rank) Mr. Hind computed the elements of its orbit. The result was in remarkable accordance with the elements deduced by our ablest astronomers from a consideration of ALL the recorded measures of *γ Virginis*. This must be accepted as decisive testimony to the accuracy of Admiral Smyth's measures, as well as to his sustained vigilance and skill through a period of thirty years.

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The same volume contains a history of all the circumstances attendant on the discovery of the planet *Neptune*, and from the fact of his being President of the Society during the warm and animated discussions which this discovery gave rise to, he had ample opportunities of hearing and weighing every point in the controversy. For close logic and terse statement of fact the article in question can hardly be excelled in the circle of astronomical literature, and it is impossible to rise from its perusal without a conviction that it is the production of a fearless and able man, anxious only for the truth.

The Admiral's last astronomical work was a little *brochure* entitled *Sidereal Chromatics*, which also may be considered as supplementary to the *Cycle*. It contains many curious facts and interesting speculations on the subject of the colours of stars, and a first attempt to verify the tints by reference to a distinct and well-defined chromatic scale.

Such was Admiral Smyth as an observer and astronomical writer. It is not our purpose to trace his career except as an astronomer, and, as a member of our Society, further than is necessary to a correct appreciation of the MAN. He has left his mark on all he touched. As a geographer, a hydrographer, a numismatologist, and an antiquarian, he was equally distinguished by the depth of his inquiry, his untiring industry, and the sagacity of his deductions. A reference to his works on the Mediterranean and on Ancient Medals, will at once satisfy the inquiries on these points.

As a member of the Royal Astronomical Society his memory will ever be revered. He joined the Society in 1821, very shortly after its first establishment, and from that time to a very recent period he served it either in the capacity of Member of the Council, Foreign Secretary, Vice-President, or President. How he discharged the various duties which these several positions placed him in can now be only known fully to some of the older members of the Society. For many years he attended constantly both our Council meetings and our ordinary meetings. As President during the exciting period before alluded to, he displayed to advantage the many admirable qualities of his mind, and by his obvious fairness and impartiality, though holding strong opinions himself, he guided the Society in safety, and left on the minds of those engaged in the discussions of the period the conviction that every body and every opinion had had fair play.

He possessed most of the qualities necessary for President of a body like our Society, ample information on every subject that arose, a memory rarely at fault, courage of the highest kind, and that happy blending of firmness and good-humour which both commands and wins. It may be said, with perfect truth, that the Society has never numbered among its members one more anxious for its interests, more faithful in services, or more jealous of its honour.

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After "the Admiral" (as he has been almost invariably termed of late years) removed to St. John's Lodge, the distance from town and a tendency to bronchitis rendered his attendance at our meetings less constant than heretofore, but his interest in our welfare was unabated, and his counsel often sought and followed.

For many years he was a member of the Board of Visitors of the Royal Observatory, where he was always attentive to business, and gave his hearty support to every measure which tended to promote the efficiency and usefulness of that establishment.

As President of the Astronomical Club, he was always genial and courteous, ever keeping things in happy order, and by his ready wit and flow of humour compelling the maintenance of good fellowship.

During his residence in London, he was a leading member of various other scientific societies, and in every one he connected himself with he became at once an influential member. Of these we may enumerate the United Service Institution, of which he may be said to have been the founder. He was a fellow of the Royal, the Geographical, and Antiquarian Societies. Of late years, though with mental vigour unimpaired, and a capacity for work as strong as ever, he led a life of comparative retirement, yet preparing a MS. which is now in type. He felt the necessity of quiet after so prolonged a life of activity, and this feeling was deepened by the sudden loss of a beloved and accomplished daughter a few years ago,—a loss from which it may be doubted if he ever fully recovered.

He still occasionally came to town, but, as he expressed it, he "felt his days were over" for taking a prominent part in public affairs. During the last few years of his life he suffered at times from a painful disorder, but his general health was good, and his mind as strong as ever. In the spring of last year it became evident to his family that age was beginning to tell upon him to some extent, but it was not until within the last two or three weeks of his life that any fears of an impending change were entertained. Early in September he had an attack, from which, however, he seemed to recover in a great measure, and was able to drive out, and to move about in the house pretty much as usual. On Friday the 8th of September, he was able to take a short drive; in the evening he was cheerful as usual, and sufficiently well to be able to adjust a small telescope, and show the planet *Jupiter* to his little grandson, Arthur Smyth Flower, with whom he chatted in his usual playful manner when talking to children. This was almost the last act of his life. He retired to bed at his usual hour, and was able to undress without much assistance. A few hours afterwards he was seized with a sudden suffusion of blood on the lungs, and shortly afterwards, peacefully and without a struggle, his noble and

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gentle spirit passed to its eternal rest. It was a death such as he wished to die. He dreaded a lingering illness with gradually decaying mental and bodily faculties. Conversing with the writer of this memoir on this subject some years ago, he expressed himself with much feeling, and in his own expressive phraseology added, "I trust when the 'Fell Sergeant' does come to me he will strike home." His death occurred early in the morning of September the 9th, 1865, and a few days afterwards his remains were laid by the side of his beloved daughter in the little churchyard at Stone, near Aylesbury. He was in the 78th year of his age.

It may truly be said of him that whatever he did he did it with his might. When at his work he tolerated no interruption, but when his day's work was done he was the most joyous of companions. He had a great fondness for children, and used to fill his pockets with new half-pennies, to distribute to those he met in his daily walks. To the young astronomer he was ever ready to lend a helping hand, and when a new observatory was to be built, he was generally consulted.

His letters were very characteristic of the man,—short, sharp, always to the point, and sailor-like in phrase. His handwriting was as clear and legible as the best print, every letter was distinct, and if ever handwriting showed the character of the writer it was his.

Admiral Smyth has gone from among us, but he has left behind him a memory which will long be cherished, and an example which cannot be too closely followed. Every member of the Society will join with your Council in the expression of sympathy with his bereaved widow and family, and in their admiration of one who was both great and good.* [I. F.]

JOHN FRANCIS ENCKE, born Sept. 23, 1791, was the youngest son but one of the deacon of the Jacobi Church in Hamburg. Four years after his birth his father died, leaving the care and the education of eight children to his mother, a lady of much worth, and happily possessed of great mental energy.

The first tutor of the boy was Mr. Hipp, a gentleman possessing considerable aptitude for mathematical teaching; and to his honour be it spoken, a man who rendered valuable pecuniary assistance to the orphan and moneyless family. Hipp continued this material encouragement to young Encke even after the time that he entered the College at Hamburg, well known as the Johanneum. At this College, then under the directorship of Gurlitt, who enjoyed a high reputation for classical learning, the boy-student rapidly advanced, and in

* The writer acknowledges his obligations to Sir John Herschel and the Astronomer Royal.

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addition to considerable ability in Latin composition, his knowledge of Greek was sufficient to enable him to translate and enjoy the Lyrics of Pindar. Notwithstanding, however, this early classical training, when the time came for his entrance at the University, Encke resolved henceforth to devote his attention mainly, if not exclusively, to the study of astronomy.

But here came a very formidable impediment; there were ample funds at the disposal of a poor clergyman's son for a theological career, but none for the prosecution of so unusual a study. Nevertheless, such was the acknowledged ability, and so determined was the inclination, of young Encke, that, as is happily not unusual in such cases, all the difficulties yielded at length to perseverance, and to his great joy, in Oct. 1811, he found himself at Göttingen, and a student under the celebrated Gauss.

The very newspapers of Hamburg were at that day compulsorily printed in French; as a condescension, however, or as an insult to the inhabitants, a German translation was added; in a like spirit even the university matricula of the old "Georgia Augusta" of Göttingen had the image and superscription of Jerome Buonaparte printed upon it. No wonder then that neither Gauss nor astronomy could retain the young student at his books, but, obeying the impulse which animated the whole heart of Germany, in the spring of 1813 he took up arms and marched to Hamburg for the rescue of his country from the domination of the French. After the re-occupation of Hamburg by the foreigner, Encke entered the Hanseatic Legion, then in process of formation in Holstein and Mecklenburg, and there he served as a sergeant-major in the horse artillery until July 1814. In the autumn of this year he returned to Göttingen and to his astronomical pursuits, and for nearly twelve months continued a diligent student of subjects far more peaceable, and far more congenial to his turn of mind. Nevertheless the return of Napoleon from Elba once more finds him in a soldier's uniform, but now only for a short period, and, happily, for the last time. Waterloo and its consequences restored peace to France and to Europe, and young Encke, who in peace had no taste for soldiership and a uniform, returned, for the third time, to Göttingen and to Gauss. It was thus in the midst of these stirring and troublesome events, that the spirits of such men as Franz Encke and Wilhelm Struve were disciplined and matured.

While Encke was serving as a lieutenant of artillery in the Prussian fortress of Kolberg, he became acquainted with the celebrated Lindenau, at once astronomer and statesman, and after the completion of his studies under Gauss, he was appointed, by the influence of the former, an assistant in the observatory of Seeberg, not far from Gotha. In 1820 he became Vice-director, and in 1822 he was appointed Director, in the place of Lindenau, who returned to his political career.

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It was at Seeberg that Encke commenced and completed his important work on the "Transits of *Venus* in 1761 and 1769," published at Gotha in 1822 and 1824. He also matured his investigation of the Comet of 1680, and of the remarkable comet of short period which bears his name. Zach's *Correspondence* and Lindenau's *Zeitschrift*, about this period, contain many evidences of his talents and his industry. During his directorship of the Observatory at Seeberg he was elected an Honorary Associate of the Royal Astronomical Society, and at the time of his decease was the oldest foreign member on our list. In 1824 the Council of our Society awarded to Encke their gold medal for what Mr. Colebrooke, the President of that day, properly designated as "the greatest step that had been made in the astronomy of comets since the verification of Halley's Comet in 1759." Encke had long been on the track of his comet. In 1818 he had succeeded in identifying it with the Comet of Mechain and Messier in 1786, and again with the comet discovered by Miss Herschel in 1795, and with the Comet of Pons in 1805. The result of his investigations was, that this comet, which astronomers have agreed to designate as "Encke's Comet" (although he himself always modestly calls it the Comet of Pons), would make its appearance again in 1822, although it would not then be visible in Europe. Accordingly our Society had the gratification of presenting to Mr. Rümker their medal for its discovery at Paramatta in 1822, on the same day when they bestowed a similar mark of approbation, as we have already stated, on Encke himself, for its prediction.

It was in these Memoirs, that Encke signalised himself by his systematic and most successful application of the *principle of least squares* to a number of astronomical observations. For the method itself we are mainly indebted to Legendre and to Gauss, but for the first exhibition of its vast practical value, we are indebted to the example of Encke. His mind, indeed, seems to have been pre-eminently arithmetical, delighting in the orderly and systematic development of what otherwise and to many would seem an inextricable maze of figures. Those who knew him best consider that he probably injured the generality of his mathematical analysis by the fastidious care which he bestowed upon its symmetrical arrangement.

In 1825, at the recommendation of Bessel, Encke was appointed to the Directorship of the Observatory at Berlin; the observatory itself was both improperly situated, and inadequately supplied with instruments, but ultimately, at the suggestion of Humboldt, a new observatory was erected at the expense of the Prussian Government, Encke superintending personally both its construction and its interior arrangements. And here, for eight or ten years after its completion, he continued with much assiduity to observe both with the Transit Circle and

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the Equatoreal; but his natural tastes did not lie in instrumental observations, and after the discovery of numerous small planets by various observers, he devoted himself with much success to the investigation of planetary disturbances.

The labours of Encke in reference to the Comet which bears his name have already been referred to. Having carefully taken into account the perturbing action of the planets on this comet during several successive periods, he established the remarkable fact that there is some extraneous cause in operation which continually diminishes the comet's periodic time. This is evidently the effect which would be produced if the comet suffered a resistance from moving in a very rare ethereal medium, and accordingly this is the explanation proposed by Encke, and at present generally accepted by astronomers.

Encke has also, as already mentioned, devoted special attention to the subject of the perturbations of the Minor Planets.

In the Appendix to the *Berliner Jahrbuch* for 1837 and 1838, he expounds in detail the method of calculating these perturbations which had been long used by himself and other German astronomers, and which was originally given by Gauss. In this method the perturbations of the six elements of the orbit are computed for successive equal intervals of time by means of mechanical quadratures, and from the values of the elements thus found for any given time, the co-ordinates of the body at that time are determined.

Now this method, although a very beautiful one in theory, is attended with the disadvantage of requiring the determination of double the number of unknown quantities that are really wanted, and the calculations which must be gone through consequently become excessively long.

As the number of the known minor planets become larger, the want of a readier method of computing their perturbations became more and more pressing.

Encke was thus impelled to devise a mode of applying the method of integration by quadratures directly to the differential equations of motion of the disturbed body, and he published an account of this new method in the Proceedings of the Berlin Academy for 1851. In this Memoir he refers the place of the body to rectangular co-ordinates, and he determines the perturbations of its movements during successive short intervals of time by a direct computation of the changes produced in the three co-ordinates by the action of the disturbing planet.

He estimates that the labour of computation is reduced by the new method to less than one-half of that required by the method previously employed.

It should be remarked that Prof. G. P. Bond, in a paper which was communicated to the American Academy of Arts and Sciences in 1849, had already briefly explained a method of calculating perturbations exactly similar in principle to that

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of Prof. Encke, but the latter was totally unaware of the existence of this paper when he published his own Memoir, which enters much more fully into the practical details of the method, and gives greater prominence to the importance of it as applied to the case of the minor planets.

By astronomers of the present day it is possible that Encke may be most highly estimated for the vast improvements which he introduced into the Berlin Ephemeris. The history of astronomical ephemerides is not a little varied and curious; a concise account of it will be found in the fourth volume of the *Memoirs* of the Royal Astronomical Society, on the occasion of the Council of the Society presenting Encke, through their President, with a gold medal, for the part which he had taken in the improvement of the Berlin Ephemeris. Our own *Nautical Almanac*, at that day, viz. in 1830, had fallen or had remained greatly behind the requirements of Astronomers; but in speaking of the merits of the foreign Ephemeris, the report of the Council runs as follows: "A gold medal has been voted to Professor Encke for the superb *Ephemeris of Berlin*. It would be superfluous to dwell upon the merits of this well-known work, which, far outstripping all rivalry, must be considered as the only Ephemeris on a level with the present wants of the sciences." On presenting the medal, Sir James South, the President, adds, "With the *Berlin Ephemeris*, an observatory scarcely wants a single book; without it, every one." It would, however, be disloyal, though in any other aspect it may be needless, not to add that what has just been said of the Berlin Ephemeris of 1830, may with equal truth be predicated of the *Nautical Almanacs* from 1834 to the present date; nevertheless the first impulse came from Encke and Berlin.

Many other labours of Encke may also be found in the Memoirs and Monthly Reports of the Berlin Academy, in the *Astronomische Nachrichten*, and in four volumes of the Berlin Observations. He is also well known by the publication of several excellent speeches, and especially for a memorable *éloge* on the celebrated Bessel.

Encke visited England in the autumn of 1840, in order to be present at the meeting of the British Association, and for the purpose of inspecting the English Observatories. His account of that journey is a testimony of the deep and pleasing impression which his hearty reception in England left upon his memory.

In 1859 Encke suffered from an apoplectic fit, and foreseeing the commencement of disease of the brain, he obtained leave of absence from his observatory in the spring of 1863. In the autumn of the same year, finding a recurrence of the same symptoms, and knowing what they implied, with a brave heart, the now aged man explained his forebodings to a physician, and at once placed himself under his care in an institution for

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diseases of the brain at Kiel. At the commencement of 1864 he requested permission to be relieved from all astronomical work, and until the time of his decease, continued to live in a quiet, happy state of mind, in the midst of his family, at Spandau, near Berlin.

Encke, during the forty years of his professorship at Berlin, impressed the form and bent of his mind upon many pupils, who have ably contributed their share in the progress of astronomical knowledge. There is no greater proof of the real worth of a teacher, than when his pupils speak well and lovingly of him. They see the man in his weakness and in his strength. So it fared with Encke. They bear strong and uniform testimony to his eminent frankness and truthfulness; his labours, they say, were incessant, his recreations few; he was simple in his manners, and in all his habits temperate. Towards his coadjutors and assistants he showed a severe judgment, but he set them a severer example. A man such as this, absorbed in his work, and shutting himself away from the outer world, was likely to be sometimes abrupt, or laconic, or even incautious, in his utterances; these utterances, from their bluntness or their truthfulness, occasionally gave offence, and involved Encke in trouble. As age, however, grew upon him he became more gentle in his manners, and softer in his address; and in the presence of those whom he knew and trusted, the old man would sometimes review his own life, and urge his favourite pupils to draw from his own experience lessons of moderation and self-restraint, both in passing their judgments on the labours of others, and in the amount of labour which they felt it their duty to exact from themselves.

There occurs but one more question regarding this great and venerable man; the writer of this memoir gladly adopts this language, *great and venerable*, because they are the very words selected by men who served him long and who knew him well, and who are themselves doing good public service in their own day. It is well known that great theological activity, not to say theological strife, surrounded Encke and every other intellectual thinker in Germany; it may not, perhaps, concern us, simply as students in Astronomy, but it cannot fail to interest us as men, to know what effect this independence of thought and boldness of expression had upon the spirit of a man, whose name will for ever be associated with some of the noblest and furthest-reaching efforts of the human mind. In reply to this question, we are told by those who knew him intimately, that Encke retained through life the strength and simplicity of his early faith; and we also learn that he was heard repeatedly to say, that one of the greatest pleasures of his life was derived from the fact, that one of his sons had become a minister of the Gospel. [C. P.]

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Proceedings of various Observatories.

Royal Observatory, Greenwich.

The work of the Royal Observatory, Greenwich, during the past year has been of the usual character. The Moon has been observed on the meridian, with the transit-circle, and off the meridian with the altazimuth, at all possible opportunities. The principal planets have been observed on the meridian with the greatest regularity. In accordance with a convention between the Astronomer Royal and the Director of the Imperial Observatory, Paris, the asteroids have only been observed at Greenwich during the first half of each lunation. They have been observed at Paris during the second half of each lunation.

Considerable progress has been made in obtaining observations of the stars contained in Bessel's *Fundamenta*, which had not been previously re-observed at Greenwich since Bradley's time.

Many observations of stars have been obtained at the request of Astronomers who have required places fixed by the Greenwich Transit for their particular investigations.

The new value of the Mean Horizontal Solar Parallax 8″·94 has been adopted for use in the Observatory. It has also been adopted for use in the Imperial Observatory, Paris.

The galvanic connexions have remained in perfect order and without material extensions or changes during the past year.

The photographic self-registering apparatus for the indication of Earth currents has been brought into use, and has furnished some most interesting records during the past year.

The work of the Observatory will be seen to have been chiefly directed to the storing up materials for future use in those branches of Astronomy which fall least readily within the reach of the Amateur.

Radcliffe Observatory, Oxford.

In this Observatory no changes of an organic character have taken place either in the system or subjects of observation, or the personal staff.

The year, as regards weather fit for observing purposes, has been below the average, especially during the early and late portions of it, and this has affected the number of observations, but not to a very great extent. With the Carrington Transit Circle, there have been observed 1043 objects, including 113 observations of the Sun, 55 of the Moon, 25 of *Mercury*, and 18 of *Mars*. The observations of *Venus*, *Jupiter*, and *Saturn*, have been given up, as there seems no particular use in mul-

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tiplying observations of these bodies, those made at Greenwich being amply sufficient for all purposes to which they may be applied. It seems, however, to be desirable that observations of the position of the Sun should be generally made for the purpose of securing an independent determination of the equinoctial points of the ecliptic ; and, with regard to the Moon, it may frequently happen that an observation may be made at an Observatory on a day when clouds have prevented it at another. The same remark applies to observations of *Mercury*, which are necessarily scanty, and in which the Radcliffe Observatory has been particularly successful.

The heliometer has been used by Mr. Main for observations of Struve's *Lucidæ*, as in the preceding year; but, nearly all the stars in Struve's first Appendix, at large distances from each other, have been added to the list, together with the stars having conspicuous proper motion included in his second Appendix.

Some occultations of stars by the Moon have also been observed with the heliometer telescope, and thoroughly reduced. The heliometer is now in a very complete state of repair and adjustment. The error of elevation of the areas which existed in former years has been accurately corrected, and a much simpler means of conveying the galvanic current for the illumination of the scale has been adopted. Failures of the light rarely occur now, and when they do happen, they are generally traceable to the battery, to which a very simple and easy test is applied.

The meteorological observations and discussions have been kept up with the same vigour and success as in former years ; and in the records of recent gales, which have been unusually protracted and severe, a good stock of materials exists for comparison with other Observatories, with a view to the advancement of the theory of storms.

The reductions of the astronomical observations are in a very advanced and satisfactory state, Mr. Main having devoted a great portion of his own time to them.

The reductions of the astronomical observations of 1864 are completed, as well as the Right Ascensions of 1865, excepting in the latter case the completion of the reductions to mean right ascension of the stars not used for clock or azimuthal error. The Right Ascensions of 1864 have also been written out into ledgers, and compared with the B. A. C. and other Catalogues; and the catalogue of all stars observed in 1864 (including 1208 stars) has been formed.

All the observed Right Ascensions of planets to the end of 1865, have been compared with the *Nautical Almanac*, and the mean times have been computed.

The printing of the astronomical portion of the volume of the *Radcliffe Observations* for 1863 is very nearly completed; and Mr. Main was enabled, some time since, to distribute a

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few copies of the Catalogue of 1115 stars observed in that year.

A little has been done additionally towards the completion of the Catalogue of Stars observed between the years 1854 and 1861, but the necessity of providing for the daily routine of work, and the reduction of the observations, taxes the energies of the small personal establishment of the Observatory to such an extent, as to leave little time for other subjects which admit of some delay.

Cambridge Observatory.

The objects kept mainly in view in the Cambridge Observatory for the year 1865 are —

1. A determination with the meridian instruments of the stars to which M. Oeltzen has assigned considerable proper motions, by a comparison of the *Histoire Céleste* with Argelander's Northern Zones. The original intention of obtaining five independent observations of each star with the transit instrument and mural circle has been adhered to, and a large proportion of the requisite observations is now completed.

2. The observations of Argelander's list for standard stars, given in the *Ast. Nach.* No. 1540.

3. The usual observations of the Sun about the times of the equinoxes and solstices, and of the larger planets at opposition.

4. The Northumberland Equatoreal has been employed chiefly in observing Faye's Comet, seeking for Biela's, observing the occultations of stars by the Moon, and examining the surface of the Sun. Suitable improved eye-pieces have been provided and used for these purposes.

5. The reductions of the observations, which had fallen considerably into arrear

The means of the transit and circle observations are taken, and the corrections for irregularity of pivots in the former and for runs in the latter are applied to the latest date of the observations. All the small corrections for the Transit Instrument are applied; the clock errors, obtained to the end of August 1865, and the true north polar distances to the end of 1864.

Some changes have been made in the observations, furnishing data for instrumental corrections. The pivots of the Transit Instrument have been carefully examined through the entire revolution, at equal intervals and at important points, and some interesting results are the consequence. The collimation errors are obtained independently of reversal, very frequently in each position of the instrument, by the aid of Bohnenberger's eyepiece, and by direct and reflection observations of slow stars, chiefly *Polaris* and *δ Ursæ Minoris*. In fact, this and all the

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other small corrections are found out, as far as possible, every night that is favourable. As there are frequent and irregular changes in the runs of the microscopes of the Mural Circle, it has been the practice for some time to take the runs with each observation.

Royal Observatory, Edinburgh.

Mr. Piazzi Smyth states that the only instrumental changes in the Royal Observatory, Edinburgh, since the last Report, have been two clock-improvements of local invention. The first is a dynamical regulation of ingenious construction by Messrs. Ritchie and Son, to the escapement of the "loud sidereal seconds-striking clock," for the equalization of its adjacent beats; which are now brought by this contrivance as close to perfection in that way, as the transit-observer can possibly desire. The next is a new, and in practice most satisfactory, method by Mr. Lang, both for first bringing a normal mean-time clock to a small rate without stopping it to make alterations on the pendulum, and then correcting its indications each day, if required, to any small fraction of a second without interfering with the goodness of its rate during the remaining twenty-four hours.

Observations of star-places with transit instrument and mural cirele have been going on as usual; together with the furnishing of the public with true time through means of the three several methods of time-ball, time-gun, and controlled-clocks.

The reduction of meteorological observations for the Registrar General of Scotland has also been in continual progress, and occupies a large share of time, the number of observing stations being fifty-five; and it is only too apparent that until meteorological theory is very much improved, the number cannot be reduced.

Thanks to the excellent performances of their duties by the two assistant astronomers, the above never-ending tasks were not sensibly interfered with by the absence of the Astronomer Royal of Scotland in Egypt during several months of last winter and spring. This visit had resulted from an inquiry which circumstances had called on him to take up connected with the principal observational facts, many of them astronomical, of the Great Pyramid; an inquiry merely literary at first, but soon demanding an actual repetition of the chief observations at the place; so that it was rather fortunate it should have fallen on the only Director of an Observatory who is officially non-resident.

The observations,—which were much facilitated by the liberal condescensions of His Highness the Viceroy of Egypt, and which acknowledged the importance of the three great epochs of modern investigation there, viz., Professor Greaves in 1639,

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the French *savans* in 1799, and Colonel Howard-Vyse in 1837, —were mainly directed in three departments of linear, angular, and thermal measures, with special reference to all cases of discordance amongst former observers, as well as the recovery, where possible, of ancient fiducial marks or surfaces; in connexion with which it may be mentioned, that the astronomical bearings of the corner sockets cut in the natural rock of the hill, marking the original size of the finished monument, were determined on two sides of the Pyramid by means of a powerful altitude-azimuth circle reading by microscope micrometers. This instrument was likewise carried to the top of the Pyramid, and also into the interior, and wherever there were important angles requiring accurate measurement.

The investigation lasted through 110 days of continued work on the spot, and its records are now in course of preparing for publication.

Glasgow Observatory.

The operations at the Glasgow Observatory during the past year have been mainly of the same character as in former years. The transit circle is still employed in the observation of stars included chiefly between the sixth and ninth magnitudes. A few observations of the minor planets have also been made with the instrument. The Ochtertyre Equatoreal has been used to a great extent in micrometric measurements of double stars, a list of such objects having been selected for the purpose from Struve's great Catalogue. The chief novelty of the operations during the past year has been the determination of the longitude of the Observatory, by means of galvanic signals exchanged on several nights with the Royal Observatory, Greenwich. An account of this operation, the result of which was very satisfactory, has been recently published in the *Monthly Notices*. The arrangements for the transmission of true time to the city and port of Glasgow which have occupied so much of Professor Grant's attention during the last two years are now established on a permanent footing. Jones's invention for regulating clocks by electricity is used on an extensive scale in connexion with the Observatory, and the results afford a complete confirmation of Mr. Hartnup's previous experience, as regards the admirable precision and practical utility of the method.

Liverpool Observatory.

At Liverpool, Mr. Hartnup has been engaged during the past year in making arrangements previous to the removal of the Observatory from the Waterloo Dock pier-head to Bidston, about three miles west of its present position. Considerable delay has been caused by the excavation of stone for the new

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building, from the site on which it is now being erected. Some practical good will result from this having been done, as the underground accommodation which has been provided will be of great advantage for all investigations requiring uniformity of temperature. The space around the building is well adapted for placing the various meteorological instruments in such positions as to insure results uninfluenced by surrounding objects.

Preparations have been made for determining the difference of longitude between the old and new stations, and observations will be commenced as soon as the new transit-pier is covered in, so as to protect an instrument which will be temporarily mounted there. The results of thermometrical observations taken at both stations during the last twelve months show that the temperature of the air is lower, and the daily range much larger, at Bidston, than it is on the Waterloo Dock pier-head.

The establishment of a depot near the margin of the river for the reception and safe custody of chronometers, and for supplying such rates as can be obtained during the uncertain time that ships remain in port, has not yet been decided on. The necessity for such a provision would be greatly diminished if all chronometers were tested periodically, and mariners supplied with tables of existing errors.

The Greenwich mean time can be obtained with so much facility from the time-balls and clocks in Liverpool, that there is no difficulty in finding the rate in the temperature that prevails at the time; but without a knowledge of the corrections due to change of temperature, it is impossible, when the rate is found in a low or a medium temperature, to say what the alteration will be when the timekeeper is exposed to the heat of a tropical climate.

Kew Observatory.

Mr. Balfour Stewart, Director of the Kew Observatory, reports:—

The Kew Photo-Heliograph has been uninterruptedly at work under the direction of Mr. De La Rue since the last report, and during the year 1865 as many as 272 pictures of the Sun have been taken. The numbers of groups observed in Kew and in Dessau will in future be annually published in the *Monthly Notices* of the Royal Astronomical Society in the same manner as Hofrath Schwabe has done for the past forty years in the *Astronomische Nachrichten* with such benefit to science.

The work of measuring and reducing the Sun observations, as carried on in Kew, has principally two objects in view; first, to bring out in a proper way a great number of facts calculated to throw light on Solar Physics, and, secondly, to collect as many accurate observations as possible, in order to

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improve the assumed elements of rotation of the Sun, a problem which the recent and beautiful researches of Carrington have rendered more important than ever. A first instalment of results bearing on the former part of the work has been published very recently under the title "Researches on Solar Physics by Warren De La Rue, Balfour Stewart, and Benjamin Loewy, Series I." The results of this investigation appear to confirm the hypothesis of Wilson, in which spots are regarded as hollows in the luminous surface of the Sun; and they also appear to render it probable that the various degrees of luminosity observed on the Sun's disk are all due to one cause, namely, the presence to a greater or less extent of a comparatively cold, absorbing atmosphere. The second series, which is based on measurements of the areas covered by the nuclear and penumbral parts of all spots, is in active progress. This part of the work embraces the whole of Mr. Carrington's and of the Kew pictures, and will not only be the groundwork of the immediate investigation for which it is intended, but promises to extend the foundation for those inquiries which connect magnetical with solar phenomena.

The Kew researches have greatly gained in scope by the munificent present of all his observations by Hofrath Schwabe, who desired to make them available for the Kew observers, and also by the liberality with which Messrs. Carrington and Howlett have lent their excellent records, thereby placing a most unique collection of material at the disposal of the observers. The access granted to the authors by the Royal Astronomical Society to the observations of Pastorff and Shea will also doubtlessly prove of great advantage.

For the purely astronomical part of the work which the Kew Observatory has undertaken, a great many pictures have already been measured and reduced; and since great care is taken to perfect the method of observation and render the photographic record as good a basis for calculation as the most delicate astronomical observation, there is every reason to hope that this part of our knowledge will also derive benefit from the labours of the Kew observers.

A new collimator arrangement for testing sextants, devised by Mr. Cooke, is in course of construction under the superintendence of Mr. Francis Salter, and will shortly be erected at the Observatory, so that nautical men will then be able to have their sextants tested in an accurate manner. It is also the intention of the Kew Committee to offer to travellers and scientific observers an opportunity to make themselves acquainted at the Observatory with portable astronomical instruments of every kind, with the mode of using them properly, and the best methods of reducing observations. It is unnecessary to dwell on the great benefit which it would confer on astronomical science, if good use could be made of such an opportunity.

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The fundamental determinations for the series of pendulum observations to be made in India in connexion with the great Trigonometrical Survey, and for which the Kew Observatory will form the base-station, have been completed, and a report of the experiments, submitted to the Royal Society by Messrs. Balfour Stewart and Loewy, has been published. The instruments have arrived in the best state in India, and the communications which these gentlemen have received from Colonel Walker and Captain Basevi show that the work there has already begun and is actively carried on.

Mr. De La Rue's Observatory.

At Mr. De La Rue's Observatory at Cranford the ordinary routine work has been carried on, and photographs of the Moon in certain phases have been made from time to time, in order to fill up lacunæ existing in the extensive series already procured. On October 4th photographs were obtained both before and during the Eclipse of the Moon, which was nearly in a state of mean libration; the pictures procured before the eclipse are now at the disposal of Mr. Birt, for the preparation of the skeleton chart of the British Association. The pictures obtained during the eclipse were found to be in stereoscopic relation with those of February 1858, so that when they are combined with each other, stereoscopic pictures of phases of a lunar eclipse are presented.

A diligent but ineffectual search was made by Mr. De La Rue and his assistant, Mr. Reynolds, for Biela's Comet on several occasions, on some of which the nights were remarkably fine. The sweeps were so extensive and so carefully gone over, that if the comet had been visible with a 13-inch reflector, it would have been discovered, even if considerable errors had existed in the ephemerides.

An Observatory has been built in a position $3''{\cdot}6$ to the north and $0^s{\cdot}23$ to the west of the old Observatory, in which has been erected the Equatoreal formerly belonging to the late Mr. Palmer. The object-glass of this telescope, by Merz, proves to be a very fine one, but the stand is far too weak, and will have to be replaced by a better mounting. The entire Observatory, walls and dome, 12 feet in diameter, revolves easily on a rail fixed to the floor: it is octagonal in form; two broad ribs extend from one octagonal face to that opposite to it, and carry the rails on which the curved shutter travels; this shutter, 2 feet 6 inches in the clear, is moved by a drum, and a handle carrying a pinion which gears into the wheel of the drum.

Some years ago Mr. De La Rue made photographs of the Sun on a large scale (three feet in diameter) by means of his

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reflector and a secondary magnifier; the working of this apparatus presented some difficulties, and he has now having constructed by Messrs. Cooke and Sons, of York, a refractor for that purpose, 13 inches in diameter and 10 feet focal length. This instrument will be specially corrected for the photographic rays, and will serve for taking lunar photographs in the principal focus, as well as Sun pictures, after the rays have passed through the secondary magnifier. It is anticipated that the Sun pictures procured with the new instrument will set at rest many disputed points in Solar Physics.

Lord Wrottesley's Observatory.

At Wrottesley the past year has been mainly devoted to the reobserving such of the double stars contained in Lord Wrottesley's Catalogue of 398, as have rapid orbital motion, or possess other features of interest. The new driving-clock constructed on the model of that of Mr. Dawes, which has been applied to the Equatoreal, is very successful, and the Equatoreal itself has been much improved by the alteration referred to in our *Memoirs*, vol. xxix., p. 86. Although Meteorology does not fall within the province of our Society, it is not out of place to mention that a most valuable communication was made by Mr. Follet Osler to the British Association at their Meeting at Birmingham, describing the results of a careful comparison and discussion of the Anemometer records of Wrottesley and Liverpool. Those persons specially interested in this subject will have an opportunity of reading the paper, which has been ordered to be printed *in extenso* in the Reports of the British Association.

Mr. Fletcher's Observatory at Tarnbank.

This Observatory is probably entitled to rank among the best and most complete observatories in private hands in existence. The chief instrument is a very fine 9·5-inch refractor of 12-feet focus, mounted on a long polar axis, which is probably unique,—it is a single block of cast-iron. The mounting is very fully described in the *Notices* for last June, and need not be further alluded to, except to record that, in firmness and ease of motion, it is completely successful. It is covered by an elliptic dome 18 feet diameter. The other instruments are a 30-inch transit by Simms, and an excellent clock by Frodsham. We are informed that Mr. Fletcher is engaged in re-observing the *Bedford Catalogue*, a work which will occupy some considerable time; and when that is done, it is his intention to bring out a new edition of Admiral Smyth's

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Cycle of Celestial Objects, for which purpose the Admiral, some time ago, made over to him his entire interest in that work.

Mr. Huggins's Observatory.

Mr. Huggins has continued during the past year the prismatic researches, some of the results of which were given in the last Annual Report.

On January 4, 1865, he observed the disappearance of *s Piscium* at its occultation by the Moon. The result was negative as to any extensive atmosphere surrounding the Moon.

Mr. Huggins has analyzed the light of about 40 Nebulæ and clusters, in addition to those described in the last Report. All these objects give, with his telescope, either a continuous spectrum or a spectrum consisting of one, two, or three bright lines. These bright lines occupy the same positions in the spectrum as the bright lines of the nebulæ which he first examined. When the light of a Nebula is dispersed by the prism into a spectrum consisting of light of all refrangibilities, the spectrum is extremely faint. On this account it has not been possible to ascertain whether the continuous spectra, which some nebulæ give, are crossed with dark lines, as the solar and stellar spectra are.

The seven nebulæ which follow give a spectrum of one, two, or three bright lines. Some have in addition a faint continuous spectrum. These gaseous bodies are—

No. 2102	27 H. IV.	No. 4572	16 H. IV.	No. 4627	192 H. I.
4234	5 ♇.	4499	38 H. IV.		
4403	17 M.	4827	705 H. II.		

The following nebulæ and clusters have continuous spectra:—

No. 105	18 H. V.	No. 4159	1945 *h*.	No. 4485	56 M.
307	151 H. I.	4230	13 M.	4586	2081 *h*.
575	156 H. I.	4238	12 M.	4625	51 H. I.
1949	81 M.	4244	50 H. IV.	4627	192 H. I.
1950	82 M.	4256	10 M.	4600	15 H. V.
3572	51 M.	4315	14 M.	4760	107 H. II.
2841	43 H. V.	4357	190 H. II.	4815	53 H. I.
3474	63 M.	4437	11 M.	4821	233 H. II.
3636	3 M.	4441	47 H. I.	4879	251 H. II.
4058	215 H. I.	4473	Auw. N. 44.	4883	212 H. II.

Mr. Huggins has made an attempt to determine approxi-

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matively the intrinsic brightness of three of the gaseous nebulæ. It is probable that these bodies consist of continuous masses of material. In the telescope they present surfaces subtending a considerable angle. As long as a distant object is of sensible size in the telescope its original brightness remains unaltered. By a suitable method of observation the intensities of these nebulæ have been obtained in terms of the light of a sperm candle burning at the rate of 158 grains per hour.

The Light of Nebula 4628 1 H. IV. = $\frac{1}{1508}$th part of that of the Candle.
,, Annular Nebula in *Lyra* = $\frac{1}{8032}$th ,, ,,
,, *Dumbbell* Nebula = $\frac{1}{19604}$th ,, ,,

This estimation, in each case, refers to the brightest part of the nebula. Nebula 4628 gives a spectrum of three lines, and also a faint continuous spectrum. The nebula in *Lyra* and the *Dumbbell* nebula give one bright line only. These values are only approximative, and are too small by the unknown corrections for the possible power of extinction of space, and for the absorptive power of the Earth's atmosphere. Similar observations made at intervals of time may show whether the brightness of these strange bodies is undergoing increase, diminution, or periodic variation.

On January 9, 1866, Mr. Huggins observed the spectrum of Comet I. 1866. The comet appeared in his telescope as an oval nebulous mass, surrounding a small and dim nucleus. The prism showed that the nucleus was self-luminous, that it consisted of matter in the state of ignited gas, and that this matter is similar in constitution to the gaseous material of some of the nebulæ. The coma shines by light which has emanated from another source. Since the extremely diffuse matter of the coma cannot be supposed to contain solid or liquid matter at the high temperature necessary for incandescence, it seems almost certain that the coma reflects the Sun's light. On this supposition, the prism gives no information whether the material of the coma is solid, liquid, or gaseous. Terrestrial phenomena suggest a condition similar to fog or cloud. If the luminous gas of the nucleus suffers condensation and subsequent diffusion to form the tails of comets, it must pass through a condition in which it neither emits nor in any large degree reflects light. Dark spaces are frequently seen between the envelopes of comets.

The observations in full, of which the results are given in this Report, have been communicated recently in two papers to the Royal Society.

Mr. Birt continues his observations on the Moon's surface at Hartwell and London, as mentioned in the last Report.

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During the past year four forms have been issued by the Lunar Committee of the British Association for the advancement of Science, viz. No. 1 for the reception of general observation of the Moon's surface; No. 2, a Table of areas for assisting in symbolizing objects; No. 3, for recording numerical and other data, appertaining to each object symbolised; and No. 4, for aiding in the computation of positions of the second order. These forms are found to be very useful, and by means of them nearly 800 objects have been symbolized and entered, and are now in progress of being inserted in an outline map of 100 inches to the Moon's diameter, on which the co-ordinates of each point determined by previous selenographers, especially Lohrmann, and Beer and Mädler, are being set off in parts of the Moon's semi-diameter to the fourth place of decimals and the excellent photograph taken by the President, Mr. Warren De la Rue, just after the lunar eclipse of October 4, 1865, and near the epoch of mean libration, is employed in the delineation of the forms of objects. As every principal object on the photograph will be transferred by measurement to the Map, it is expected that a degree of accuracy will be attained far beyond that which a filling in by eye-sketching can possibly accomplish. The most accurate determinations of lunar positions are those of the first order, but as these are comparatively few and the triangles formed by them large, amateurs might assist greatly by increasing their number. It is intended to lithograph portions of the Map now constructing and to distribute them to observers for this and kindred purposes.

Progress of Astronomy.

It is extremely difficult, if not impossible, to estimate correctly the amount of actual progress made in a science such as Astronomy during any single year. Not seldom the real work lies for the moment either not wholly matured, or by its very nature is to be stored away as seed for the harvest of future years. All that can be done in this direction is to give, so far as may be, a faithful record of what has actually come to the surface, and has already found a place in the scattered annals of learned Societies.

Cometic Structure.

Among the most remarkable and, until a year ago, certainly among the most unexpected accessions to our knowledge, is that which this year has come to us latest in the order of

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discovery. We refer to Mr. Huggins' observations on the spectrum analysis of the light from a comet. The light of the nucleus of Comet I. 1866, as examined under his instrument, gives a spectrum consisting of but one bright line, whereas the spectrum formed by the light from the coma gives a spectrum which is continuous. The inevitable conclusion to be drawn from these observations is opposite to that which our prepossessions would have led us to expect, inasmuch as, consistently with the present state of our physical knowledge, we are forced to conclude that the light of the cometary nucleus examined by Mr. Huggins must have emanated from a gaseous source; whereas, guided partly by other physical considerations, no doubt remains that the coma contains fluid or solid materials. Thus the suspicion of analogy between cometic and nebular matter has received this further confirmation. No doubt difficult observations of this nature require repetition, but the known caution and experience of the observer invite our confidence.

Biela's Comet.

It is possible that in some unknown relation to the structure of these bodies thus revealed, may stand the fact of the non-apparition of Biela's dichotomized Comet, up to the present date, when the two bodies must have passed their perihelion. We are indebted to the labours of Mr. Hind and to the liberality of Mr. Bishop for the regular supply of ephemerides of this remarkable system, but hitherto with no results: had the comets been visible in Europe, apparently they must have been detected in the careful and extensive sweeps made with such instruments as the 15-inch object-glass at Pulkowa, and the 13-inch mirror at Mr. De La Rue's Observatory.

Hoek's Cometic Hypothesis.

Before dismissing the question of Comets, it seems desirable to call attention to two interesting papers written by Mr. Hoek, of Utrecht, on the subject of these bodies, and which have been inserted in our *Monthly Notices*. Mr. Hoek considers that he has advanced mathematical grounds whereon to found the probability that every star is associated with a cometary system of its own; but that, owing to the attraction of planetary or other cosmical matter, these bodies continually leave their proper primaries and revolve, either permanently in ellipses, or temporarily in parabolas or hyperbolas, round other Suns. Should this hypothesis of Mr. Hoek's be found, on examination, tenable, a new view of these erratic and intractable bodies will be opened.

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Retardation of the Time of the Earth's Rotation.

A communication has been recently made by M. Delaunay to the Academy of Sciences at Paris, on the difficult question of the acceleration of the Moon's mean motion. M. Delaunay thinks that he can satisfactorily account for that outstanding part of the acceleration which at present appears not to be accounted for by planetary disturbance.

On the hypothesis that the disturbing forces of the Sun and Moon act on the lagging protuberance of the great tidal wave, he considers that the amount of this action is quite sufficient to produce a progressive augmentation in the time of the Earth's rotation on its axis, sufficient to account for the outstanding 6″ of the Moon's acceleration. Should this hypothesis eventually, and on re-examination, prove to be correct, it will be worthy of remark, that the scrupulously exact methods of astronomical investigation during the last few years, will have enabled us to estimate with greater accuracy two of the prime elements of the solar system, viz., the mean distance of the Sun from the Earth, and the length of the terrestrial day. Whether this be the case or not (and no opinion is here hazarded upon the subject), this remarkable conclusion of M. Delaunay can scarcely fail to give an additional impetus to the reconsideration of the more difficult and obscurer parts of the Lunar Theory.*

Variation of the Eccentricity of the Earth's Orbit.

In connexion, again, with the alteration in the eccentricity of the Earth's orbit, so intimately related to the former question raised by M. Delaunay, stand some remarkable speculations very recently raised or renewed by Mr. Croll relative to the alleged effects of climatal heat or cold produced in the course of many thousands of years by the variation in the aphelion and perihelion distances of the Sun from the Earth. Geologists give us ample evidence of at least one glacial period, and they are now beginning to observe indications of a succession of these periods of refrigeration, separated from each other by very long intervals of time. No doubt during the course of these immensely separated epochs there have been cycles of change in the eccentricity of the Earth's orbit, and it will be the province of mathematicians and physicists, possessing competent skill, to determine how far such a cause is sufficient to account for a succession of Glacial Periods.

* Since the above was in type, it is understood that the Astronomer Royal has investigated the effects of the Moon's action on the tidal wave, with results not in accordance with those of M. Delaunay. *Adhuc sub judice lis est.*

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Solar Physics.

There has been much activity exhibited during the past twelve months in this interesting and important field of research, and more especially at Kew. The investigations there made, in addition to other results not yet published, confirm the hypothesis of the cavernous nature of solar spots, and of a downward rush of colder matter from above. In addition to the observations made at the Observatory itself, Messrs. De La Rue, Stewart, and Loewy, have availed themselves of all the drawings and the investigations on the subject within their reach. Especially they are measuring the areas of the spots on the solar surface, as depicted in Mr. Carrington's work, inasmuch as they conceive that the total amount of spot-area on the Sun at any assigned time, must be at least one important exponent of solar activity at the moment in question. In their confirmation of the truth of Wilson's hypothesis of the cavernous nature of solar spots, and in their own view of the downward rush, they are strengthened by the investigations of M. Chacornac, to whose successful labours on Solar Physics, astronomers are much indebted. The attention of observers will be amply repaid by a reference to the notices of M. Chacornac's labours published in the *Comptes Rendus* of the French Academy, in relation to the reflective power and the variable luminosity of the different portions of the photosphere of the Sun, and to the successive envelopes which appear to surround that body.

The Rev. J. Howlett, Mr. Lockyer, Professor Phillips of Oxford, and Canon Selwyn of Cambridge, have also made many valuable contributions in the same direction.

M. Faye, from the result of his investigations on the proper motions of Mr. Carrington's spots, comes to the same conclusion regarding the downward rush; but he dissents from the hypothesis of the Kew observers, that the deficient luminosity of a solar spot is owing to a diminution of temperature; on the other hand, it does not seem possible to harmonize this objection with the present state of our knowledge of radiant heat.

Again, M. Faye, in a Memoir recently communicated to the French Academy, on applying to the heliographic longitude of a Sun-spot the correction due to the parallax which would necessarily arise from its elevation above, or its depression beneath the surface of the photosphere, removes certain apparent anomalies which otherwise exist in the proper motions of the Sun-spots contained in Mr. Carrington's work. The following very interesting conclusions are deducible from M. Faye's mathematical investigation:—

1. Sun-spots are depressions beneath the surface of the Sun's photosphere, varying in depth from about $\frac{1}{100}$th to $\frac{1}{200}$th of the Sun's radius, i.e. from about 40,000 to 20,000 miles.

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2. Many apparent *irregularities* in the proper motions of Sun-spots hitherto supposed to be capricious, or attributable to cyclones or tornados, or to their own mutual actions, are now probably explicable by the continued variation in the motion proper to each successive parallel of the photosphere.

3. The astonishing *regularity* in the motions of Sun-spots, the maintenance of which is thus demonstrated by M. Faye, appears to that astronomer incompatible with any hypothesis of mere superficial or local movements in the photosphere, but seems to point to some more general action arising from the internal mass of the Sun.

Professor Spoerer of Arlem in a memoir submitted to the Academy of Sciences in Berlin, has obtained a formula which (from four years' observations) expresses the law for the dependence of the period of the Sun's rotation on the latitude

$$\xi = 16^{\circ}{\cdot}8475 - 3^{\circ}\,3812 \sin (41^{\circ}\,13' + \text{Heliographic latitude.})$$

where ξ is the angle of rotation in a day.

M. Spoerer, though as it seems on insufficient data, calls in question Mr. Dawes's conclusion relative to the rotation of the darkest portion of a spot. Some observations of Padre Secchi appear to confirm the existence of those peculiar appearances on the general surface of the photosphere which were first described as willow-leaves by Mr. Nasmyth.

In addition to the establishment of a photoheliograph at Wilna, there is a prospect of the erection of a third at Quebec. If this hope is realized, there will then be a station in England, in Russia, and in America, by means of which, on account of the difference of longitude, we may hope to have an almost uninterrupted self-register of solar phenomena.

Telescopic Diameter of the Moon.

Some important calculations have been made from occultations of Stars by the Moon as observed at Greenwich and Cambridge, with a view of ascertaining the difference between the semi-diameters of the bright and the dark Moon. The result is that the Greenwich instruments give a telescopic semi-diameter too large by about 2″. Mr. Airy considers it probable that the whole of these 2″ is due to irradiation; and he remarks that, even if the whole of it were supposed to be caused by a lunar atmosphere, its attenuation must be so great that it would probably be discoverable by no other mode of observation. It may here be worthy of remark, that accurate measurements of Mr. De La Rue's photographs of the great Solar Eclipse of 1860 give precisely the same results; a circumstance which leads to the hope that photography may ultimately become a valuable auxiliary in even micrometrical observations.

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Meteoric Astronomy.

Meteoric Astronomy is gradually being brought within the domains of known physical law. Regular observations, whenever the state of the atmosphere admits, are made at the Royal Observatory, at all the epochs of meteoric activity which have as yet been established or suspected. The well-known periodic star-shower of November was very diligently and successfully observed at Greenwich under Mr. Glaisher, at Cambridge under Prof. Adams, and at Hawkhurst by Mr. Alexander Herschel. Very complete preparations having been made at these three places of observation, the results possess unusual interest and claim our confidence. A more detailed account will be given in the *Monthly Notices* of next March.

Balloon Ascents by Night.

Some very remarkable results have been obtained from meteorological observations made by Mr. Glaisher during his intrepid ascents in balloons by night, which may have an important bearing both on the theory of astronomical refraction and on the theory of heat.

Mr. Glaisher observed that the decrease of temperature owing to increase of elevation, was variable throughout the day, but about sunset the temperature remained constant within the limit of an elevation of 2000 feet. This observation suggested the probability that after sunset the temperature might even increase with increase of elevation, at all events within the same limit of height. Hence arose the proposition for nocturnal ascents. Mr. Glaisher found that with the aid of a properly constructed Davy lamp he could read the instruments with sufficient facility, and in December last he made two ascents, and the results have justified the amount of trouble, and even of danger encountered in the enterprise. In the first ascent, when the sky was cloudless, the temperature *increased* with increase of elevation. During the second ascent, when the sky was overcast, there was a *small decrease* of temperature as the height increased.

In ascents by day, Mr. Glaisher found that the difference of the readings of two thermometers, the one having a blackened bulb exposed to the Sun, and the other shaded from the Sun, continued to *decrease*, as the elevation increased, until at the height of about five miles, when the readings of the two thermometers became identical. Mr. Glaisher considers that the nocturnal observations deserve repetition and extension.

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Variable Stars.

Mr. Chambers has collected from various sources, and has published in the *Monthly Notices*, an interesting and valuable Catalogue of stars up to this date known to be Variable. It is herein that an ample field is thrown open to those non-professional astronomers who possess the requisite means at their private observatories. The alleged variations in the great nebula in *Orion*, and especially the anomalies in the visibility of some of the small stars in the trapezium recently indicated by Mr. Huggins, will furnish scope for employment of telescopes of various apertures.

Mr. J. Gurney Barclay has recently printed a volume of observations made at his Private Observatory, Leyton, during the years 1862–64, for which he merits the thanks of all persons interested in astronomy. Probably the observations of Minor Planets and Comets published in this Catalogue are unique as proceeding at this day from a private observatory. Mr. Barclay very handsomely acknowledges the assistance he had derived from M. Romberg, now attached to the Royal Observatory, Berlin.

Dr. Brünnow, lately of the Observatory Ann Arbor, U.S., has recently been appointed to succeed the late Sir W. Rowan Hamilton at Dublin, as Astronomer Royal for Ireland.

Papers read before the Society from February 1865 *to February* 1866.

1865.

Mar. 10. On the Spectrum of the Nebula in *Orion*. Prof. Secchi.
Observations of Comet I. 1864. Mr. Tebbutt.
Opposition of *Mars*, 1864. Mr. Joynson.
Drawings of *Mars'* Opposition, 1864. Mr. G. Williams.
Account of a Comet seen in Brazil. Prof. Challis.
On the Date of a Communication of a Mode of Observing Transits without Reference to Hearing or Touch. The Astronomer Royal.
Occultations Observed by him. Capt. Noble.
Comet Observations at Sydney Observatory. Mr. Smalley.

April 12. On the Planet *Mars*. Mr. Joynson.
On an Appearance presented by the Spots on *Mars*. Mr. Talmage.

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Extract from Letter announcing Deaths of Professor Bond and Lieut. Gilliss. Dr. Gould.
On the Star *Baily Lalande* 14512 and Neb. H.N. 45. M. Schultz.
On a New Comet discovered in Australia. Mr. Ellery.
Ditto ditto Mr. Abbott.
Extract from Letter of Mr. Todd on the same Comet. Mr. Carrington.
Observation of ditto Mr. Abbott.
Ditto ditto Mr. Tebbutt.
Note on the Lunar Theory. Mr. Cayley.
On an Aluminium Bronze Transit Instrument. Col. Strange.
On the Perturbations of the Planet *Neptune*. Mr. Wackerbarth.

May 12. On the Notches or Bearings for the Pivots of Transits. Mr. Yeates.
On some peculiar Instances of Personal Equation in Zenith Observations. Mr. Dunkin.
Catalogue of Variable Stars. Mr. Chambers.
On the Comets, 1860, III., 1863, I., and 1863, IV. M. Hoek.
On the Photosphere of the Sun. Mr. Fletcher.
On a Double-Prism Eye-piece. Rev. W. R. Dawes.
On the Phenomena of Solar Spots. M. R. Wolf.
Discovery of a New Planet (*Beatrix*). M. De Gasparis.
Positions of Comet, 1865. Mr. Ellery.
Second Note on the Lunar Theory. Mr. Cayley.
Note on the Nebulous Star 45 H *IV. Geminorum*. Mr. Knott.
Constant of Lunar Parallax. Mr. Stone.

June 9. On an Artificial Horizon. Mr. Merrifield.
Observations of Comet, 1865. Mr. Tebbutt.
Observation of Lunar Eclipse of April 11, 1865. Mr. Freeman.
Note on ζ *Herculis*. Mr. Fletcher.
Description of an Equatoreal Mounting. Mr. Fletcher.
Description of an Aperture-diminishing Eye-piece and a Photometer of Neutral Tint Glass. Rev. W. R. Dawes.
Observations on the Sun's Photosphere. Mr. Lockyer.
On the Double Star δ *Cygni*. Mr. Knott.

Nov. 10. Note on an Error of Expression in two Memoirs of the Astronomer Royal on the Correction of the Elements of the Moon's Orbit. Mr. Airy.
Radiant Points of Shooting-Stars. Mr. A. Herschel.
Observations of Solar Eclipse, Oct. 19, 1865. Lord Wrottesley.
Places of Comet I. 1865. Mr. Ellery.

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Observations of Encke's Comet. Mr. Tebbutt.
On the Comets of 1697 and 1683, 1860 III., 1863 I., and VI.* M. Hoek.
On the Great Sun-Spot of October 1865. Rev. F. Howlett.
On the Solar Photosphere. Mr. Fletcher.
Observations on Solar Craters of September 28 and October 8, 1865. Mr. Brodie.
Occultations of Stars by the Moon. Capt. Noble.
On Photographs of Lunar Eclipse of October 1865. Mr. Brothers.
On Personal Equation in Reading Microscopes. Mr. Stone.
On the Telescopic Disks of Stars. Mr. Stone.
On the Physical Constitution of the Sun. M. Chacornac.
On Lambert's Theorem. Mr. Sylvester.
Elements of Comet I. 1865. Mr. Tebbutt.
Expressions for Plana's e, γ. Mr. Cayley.

Dec. 8. Observations of Comet III. 1860. M. Moesta.
Observations of Encke's Comet. Mr. Smalley.
On Prof. Krueger's Memoir on a star in *Perseus*. Mr. Carrington.
Note on ζ *Herculis*. Mr. Burr.
On the Solar Photosphere. Commander Ashe.
Geocentric N.P.D. of Moon and Moon-culminating Stars. Sir T. Maclear.
Mean R.A. and N.P.D. of Stars compared with Comet I. 1864. Sir T. Maclear.
Mean Places of Stars of Comparison, derived from Observations with the Transit Circle, and Apparent Places for Dates of Comparison with Comet I. 1865. Sir T. Maclear.
Observations of Solar Eclipse October 19, 1865. Rev. T. Chevallier.

1866.

Jan. 12. Proposition for a Telescope on the Andes. Commander Ashe.
On Sun-Spots. Mr. De La Rue and others.
Ephemeris and Elements of Comet of December 9, 1865. Dr. Donati.
Observations of η *Argûs*. Mr. Tebbutt.
On the Stars within the Trapezium of *Orion*. Mr. Huggins.
On the Diminution of Actinic Effect of the Sun near the Edge. Mr. Huggins.
Occultation of 115 *Tauri*. Mr. Talmage.
On a New Mode of Mounting Silvered Glass Specula and Diagonal Mirrors in Reflecting Telescopes. Mr. Browning.

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ADDRESS

Delivered by the President, Warren De La Rue, Esq., on Presenting the Gold Medal of the Society to Professor J. C. Adams, Director of the Cambridge Observatory.

It may be truly said of the Theory of Gravitation that, while, on the one hand, its fundamental laws are easily apprehended by the human intellect, on the other the results which the application of this theory produces are of so intricate a nature, especially when the mutual action of more than two bodies is concerned, that it requires the exercise of the highest faculties of minds specially cultivated, to trace out all the consequences which arise out of the mutual attractions of the particles and masses of matter which constitute the Universe.

No application of the laws of gravitation has given more trouble to mathematicians than the development of the Lunar Theory; frequently, indeed, in the history of physical astronomy grave doubts have been entertained respecting the possibility of accounting, by means of the grand and simple law discovered by Newton, for the perplexing inequalities to which the Moon's motion is subject, and which observation has shown to exist; and even the results of observations themselves have been called into question, when the mathematician has failed to account for the phenomena they disclose.

It is for most valuable contributions to the development of this Lunar Theory that your Council have awarded our Medal to Professor Adams. While it is extremely gratifying to me to be the medium of explaining the grounds on which this award has been made; at the same time I am deeply conscious that it would have been far better, in the interest of science, if the award had been made under the presidency of a physical astronomer specially qualified by his previous pursuits to do full justice to the works of the illustrious mathematician whose merits we desire, this day, to recognise.

The Medal has been awarded to Professor Adams for his

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investigations in respect of the Lunar Parallax and the Secular Acceleration of the Moon's mean motion.

I propose first to take into consideration the last named of these works, which was communicated to the Royal Society so far back as June 1853, and published in the *Philosophical Transactions*, vol. cxliii. part iii. page 397. It is entitled, "On the Secular Variation of the Moon's Mean Motion." No contribution to astronomical physics has given rise to more discussion in modern times than the investigation of this problem by Professor Adams; and it will be in the recollection of most now present that our *Monthly Notices* from 1858 to 1862 contain much controversial writing in relation to this subject, in which several of the most renowned physical astronomers of the day took part. A summary of the history of this famous problem is given with remarkable clearness by M. Delaunay in the *Additions à la Connaissance des Temps* for 1864;* and although I shall borrow from that paper in the course of this discourse, I would recommend the perusal of the original to all who may be desirous of acquainting themselves fully with the subject under consideration.

Let me recall to your recollection that in dealing with the perturbations caused by the Sun in the Moon's motion, we have to consider only the differences between the action of the Sun on the Moon and on the Earth. If both bodies were attracted equally and in parallel directions, then their relative motions would not be disturbed, and would be in harmony with the laws of elliptical motion as derived from the force of gravitation. But the distance of the Sun from the Moon and Earth, although great, is not so large but that the Moon is, in the course of her orbital motion round the Earth, sensibly nearer to the Sun at one time than at another; moreover, the angles which are formed by the line joining the Sun and Moon's centres with the line joining the Sun and Earth's centres, are sensible, though small. It results therefrom that the attractions of the Sun on the Moon and on the Earth are generally unequal, and act in different directions; and thus arises a disturbing action of the Sun, causing sometimes an increase, sometimes a decrease of the Moon's gravitation towards the Earth; sometimes a retardation and sometimes an acceleration of the areal velocity; also continual changes in the excentricity and inclination of the Moon's orbit, and in the position of its perigee and nodes; indeed, producing such a perplexity of inequalities that they can only be traced to their final result by discussing the effects of the disturbing forces with all the minuteness of which the resources of analysis permit. It is in the investigation of these various perturbations that the famous problem of three bodies consists, "the rigorous solution of which," says Laplace,†

* Pages 21 to 72 inclusive.

† *Exposition du Système du Monde*, 5th Edit. vol. ii., p. 65.

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"surpasses the resources of analysis; but which fortunately admits of being resolved by approximation, in consequence of certain favourable conditions in the problem, for example, the proximity of the Moon to the Earth in relation to her distance from the Sun." He goes on to say that the most careful consideration is required to disentangle those terms the influence of which is sensible, and to determine with exactness those which, although small in themselves, acquire in the successive integrations a sensible value. It will be seen hereinafter that mathematicians of the highest order are liable to overlook some of these terms, and that consequently no physical astronomer ought to allow himself to be in the least deterred from re-investigating a problem because it has passed under the consideration of men whose names are the greatest in science.

In further elucidation of this reflection, I quote the following remarks of M. Delaunay,* who says, "When the extreme complication of the questions comprehended in Celestial Mechanics is taken into consideration, it will be understood that things do not present themselves to all minds with the clearness which obtains in elementary algebra. By reason of this complication, problems which are intended to be resolved are not attacked at once in their entirety; but divers portions of the solution are successively sought for in such a manner that the complete solution may be subsequently obtained by uniting together the several parts thus separately determined. In consequence of this manner of operating, the greater number of the equations which are taken into consideration are recorded in an incomplete form; only those terms are retained which are considered to have some influence on the partial results sought for, all the other terms being unexpressed; some of these unexpressed terms may however have a much greater influence on the *final solution* of the problem than those terms which are retained. The talent of the mathematician who undertakes the discussion of such questions consists precisely in distinguishing, among the excessively numerous terms of which every equation would consist if stated at length, those which ought to be retained as having some influence on the partial results to be obtained. It will therefore be understood that a discussion may arise on the influence or non-influence of certain terms on such and such a part of the general solution, and consequently on the retention or rejection of those terms in the investigation of a part of the solution; all possibility of such a discussion would disappear if the complete solution of the question could be at once and fully undertaken, but this in the greater number of cases must be considered as beyond the power of the human mind."

I have been induced to quote from Laplace and Delaunay their opinions in regard to the difficulties of the analysis, be-

* *Additions à la Conn. des Temps*, 1864, pp. 51, 52.

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cause I am sure that during the controversy on the secular acceleration of the Moon's mean motion many persons were perplexed why any difference of opinion should arise respecting a question which in reality resolved itself into one of pure mathematics.

To return to the secular acceleration of the Moon's mean motion:—you will remember that this inequality was discovered by comparing records of ancient eclipses with observation long before theory could account for it. Halley was the first who suspected its existence; he found, on calculating the Moon's place for the epochs of certain ancient eclipses recorded by Ptolemy, by means of her mean motion computed from modern observation, that the result was such as to indicate a position in her orbit less advanced than her recorded place, and the time of an eclipse so calculated was later than that actually recorded. He inferred therefore that the angular motion of the Moon must have been accelerated since the earliest astronomical records. Halley first alluded to this phenomenon in 1693, and nearly sixty years elapsed before this suspicion of his was confirmed. Dunthorne in 1749 communicated a paper to the Royal Society which contained a discussion of all the observations calculated to throw light upon the subject; Mayer also arrived at the conclusion that the Moon's motion had continually been accelerated from the earliest records. Both these astronomers found that the same mean motion of the Moon could not satisfy both modern observations and the records of the eclipses observed by the Chaldeans and Arabs. They attempted to represent these by adding to the mean longitude of the Moon a quantity proportional to the square of the number of centuries before or after 1700. According to Dunthorne this quantity should be 10″ for the first century; Mayer made it 6″·7 in his first lunar tables,* and subsequently 9″ in his later tables. Lalande also arrived at the same result as Dunthorne, namely, 9″·886, which he subsequently fixed at 10″ from the year 1700. Bouvard and Burg also, by discussing a great number of observations in the two preceding centuries, determined the acceleration with great accuracy and confirmed the foregoing results.

Now the lunar acceleration having been incontestably established, it was an object of great interest to ascertain the cause of this inequality. Euler came to the conclusion that it could not be produced by the force of gravitation. Lagrange at a later period demonstrated that neither the figure of the Moon, nor that of the Earth, nor the direct attractions of the planets, could be the cause of the phenomenon.

To give an idea of the position of the question when it was

* It is generally stated that Mayer employed in his Tables the coefficient 7″ for the lunar acceleration, but M. Delaunay informs me that 6″·7 was really the co-efficient used by Mayer.
See *Add. à la Conn. des Temps*, 1864, p. 26.

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attacked by Laplace, I may quote from his *Exposition du Système du Monde*,* the following remarks, slightly paraphrased:—"This subject has much occupied the attention of geometers, but their researches were for a long time unfruitful, not having led to the discovery of any cause which could alter the Moon's mean motion, whether by the action of the Sun and planets on the Moon or in consequence of the non-spherical figures of the Moon and the Earth; some astronomers have in consequence come to the conclusion that the secular acceleration had no existence; others, in order to explain it, had recourse to divers hypotheses, for instance, the action of comets, the resistance of the ether, and the successive transmission of the force of gravitation. But the accordance of other celestial phenomena with the theory of gravitation is so perfect, that it could but be seen with regret that the secular acceleration of the Moon should refuse allegiance to this theory, and thus alone constitute an exception to a simple and general law which by the magnitude and variety of the objects which it embraces does so much honour to the human intellect."

After many unsuccessful attempts to account for this phenomenon, Laplace at last succeeded in mastering the difficult problem, which had baffled so many distinguished mathematicians and had escaped the sagacity of Lagrange, who (says M. Delaunay†) "had almost touched it with his finger in his investigation of the *Secular Variations of the Mean Motion of the Planets*" inserted in the *Memoirs of the Academy of Berlin*, 1783.

Laplace communicated his discovery to the *Academie des Sciences* of Paris on the 19th November, 1787. In the *Exposition du Système du Monde*‡ he thus states the cause of the secular acceleration:—"*The secular equation of the Moon is due to the action of the Sun on this satellite, combined with the secular variation of the excentricity of the terrestrial orbit.*"

In accounting for the secular acceleration of the Moon, Laplace only took into account directly, the *radial* component of the disturbing action of the Sun, which, as you know, tends, on the whole, to dilate the lunar orbit. This force, it will be remembered, does not affect the areal velocity of the Moon, but it does the angular velocity. Its effect is greater when the Earth is in perihelion than when it is in aphelion, so that the Moon's orbit is more dilated when the Earth is in perihelion; it follows that, the description of the areas remaining the same so far as this central disturbing force is concerned, the angular velocity is less and the lunar month is consequently longer in our winter that in our summer. This

* Fifth Edition, vol. ii., pp. 75, 76.
† *Addit. à la Conn. des Temps*, 1864, p. 39.
‡ Fifth Edition, vol. ii., p. 76.

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alternate dilatation and contraction of the lunar orbit, as you know, gives rise to the *annual equation*. The mean diminution of the Moon's angular velocity, caused by the radial disturbing force, depends, in some degree, on the excentricity of the Earth's orbit, and is greater or less according as that excentricity is greater or less. Hence if the excentricity of the Earth's orbit diminishes, the Moon's angular velocity will increase.

Now among the changes in the elements of the Earth's orbit produced by the attraction of the planets, there occurs this change of excentricity, which is the cause of the lunar acceleration; the semi-major axis of the Earth's orbit, or the Earth's mean distance, undergoes no permanent alteration, but the excentricity of the Earth's orbit is always varying; for ages it has been gradually becoming less, and according to Leverrier* will continue to do so for about 23,980 years from 1839, when it will have attained a minimum value of 0·003314, after which it will again increase, until it attains its maximum.

As the excentricity of the Earth's orbit becomes less each succeeding year the Earth and Moon will not approach so close to the Sun at the epoch of perihelion as in the preceding year, and the Moon's orbit will gradually contract, and consequently her angular motion will increase. Laplace says, "The action of the Sun on the Moon diminishes by $\frac{1}{179}$th her angular velocity, and the numerical coefficient of this diminution of the angular velocity varies inversely as the cube of the distance of the Sun and Earth. Now in developing the inverse cubic power of the distance in a series arranged with regard to the sines and cosines of the mean motion of the Earth and its multiples, the semi-major axis of the orbit being taken as unity, it is found that this series contains a term equal to three times the half of the square of the excentricity of the Earth's orbit; the diminution of the angular velocity of the Moon contains the product of this term by $\frac{1}{179}$th of this velocity. This product would be confounded with the mean angular velocity of the Moon, if the excentricity were constant; but its variation, though small, has an influence in the long run on the lunar motion. It is evident that it accelerates this motion when the excentricity diminishes." Ultimately this acceleration will in the course of ages cease, and the Moon's mean angular motion will then begin to decrease.

Laplace further pointed out that the motions of the lunar perigee and nodes were subject to secular inequalities dependent on the same cause as the lunar acceleration. He also proved that the Moon's orbit has always the same mean inclination to the ecliptic, and that the changes produced in the excentricity and major axis of the lunar orbit, by the change of excentricity of the Earth's orbit, are insensible.

* *Additions à la Connaissance des Temps*, 1843, p. 47.

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Laplace found that the lunar acceleration amounted to 11″·135 in a century, though, in consequence of taking into account a modification of the masses of *Mars* and *Venus*, upon which the rate of decrease of excentricity of the Earth's orbit in a great measure depends, he subsequently reduced this quantity to 10″·18. But in carrying back the calculations of the Moon's place to the period of the Chaldean observations he found that it was necessary to introduce a new term depending on the cube of the time,* so that if t denoted the number of centuries from the assumed epoch, the acceleration would be

$$10''{\cdot}1816\, t^2 + 0''{\cdot}01854\, t^3.$$

The value assigned by Laplace for the lunar acceleration accorded fairly † with ancient eclipses and modern observation, and it is a fortunate circumstance that, at the period it was published, it did so, otherwise grave doubts might have arisen as to the sufficiency of the theory of gravitation to account for the phenomenon then under anxious consideration.

Subsequently in 1820, in consequence of the powerful impulse of the *Académie des Sciences* of Paris,‡ the theory of the Moon was again investigated by M. Damoiseau, and also by MM. Plana and Carlini. They carried their approximations to a much higher order than Laplace had done, but they failed to discover that the tangential force also produced a permanent secular effect. Their investigations did not greatly change the coefficient arrived at by Laplace (10″·1816). Plana's value was 10″·58 in a century, whereas M. Damoiseau, by calculating directly this coefficient in a numerical form, arrived at the number 10″·72.

These new values for the secular acceleration of the mean motion of the Moon did not materially alter the times computed for the ancient eclipses, so that Laplace rested perfectly satisfied with the results of analysis, and was not induced therefore to investigate rigorously all the effects which would be produced by the Sun's disturbing force. With respect to the theory which ascribed the acceleration of the Moon's mean motion to the resistance of the ether in space or to the progressive transmission of the force of gravitation, he thus disposes of these hypotheses: "When only the acceleration of the mean motion of the Moon was known, it might have been attributed to the resistance of the ether, or to the successive transmission of the force of gravitation. But analysis shows us that these two causes cannot produce a sensible alteration in the mean mo

* *Exposition du Système du Monde*, 5th edit., vol. ii., p. 76.

† The accordance was not perfect, as will be seen by an examination of the Table inserted by M. Delaunay in the *Add. à la Conn. des Temps*, 1864, p. 29.

‡ At the instigation of Laplace the Academy of Sciences of Paris made the Lunar Theory the subject for competition for the Grand Prize of Mathematics.

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tions of the nodes and perigee which I have shown to depend on the same cause as the secular acceleration."

He goes on to say that the accordance between theory and observation in respect of the secular acceleration establishes with certainty the *constancy of the length of the day*; "for if the length of the day were greater now by the one-hundredth of a second than in the days of Hipparchus, the actual duration of a century would be greater by 365·25 seconds, during which time the Moon describes an arc of 173″·2: the mean secular acceleration of the Moon would in consequence be increased by 4″·38 for the first century, from the year 1801; a quantity not admissible by theory." Of course if the accordance between theory and observation, thus supposed by Laplace, does not exist, this reasoning of his falls to the ground; and it will presently be seen that M. Delaunay has recently from other considerations arrived at the conclusion that the sidereal day has actually lengthened.*

M. Hansen was the next to take up the question, he calculated the coefficient of the secular acceleration of the Moon on several occasions, and found in the first instance 11″·93 (*Astronomische Nachrichten*, No. 443, March 1842), which he afterwards reduced to 11″·47 (No. 597, May 1847).

Such was the state of the theory when Mr. Adams, in a Memoir read before the Royal Society of London on the 16th of June, 1853, and published in the *Philosophical Transactions* for the same year, pointed out an important error in the method pursued by MM. Plana and Damoiseau in obtaining the secular acceleration of the Moon. The correction of this error involved a considerable diminution in the theoretical value of this acceleration. The object which Mr. Adams proposed to himself in this Memoir was to determine the amount of the acceleration of the Moon's mean motion which was due to the cause assigned by Laplace. The problem which he undertook to solve may be accordingly stated in the following purely mathematical form. If the Earth be supposed to describe about the Sun an elliptic orbit, the excentricity of which varies slowly and proportionally to the time, while the other elements remain constant, what will be the change in the Moon's mean motion due to this change of excentricity of the Earth's orbit.

In the introduction to his Memoir, Mr. Adams thus briefly explains the principle of the method employed by Laplace in the solution of this problem:—

"It is readily shown that the mean central disturbing force of the Sun, by which the Moon's gravity towards the Earth is diminished, depends not only on the Sun's mean distance, but also, in some degree, on the excentricity of the Earth's orbit. Now this excentricity is at present and for many ages has been diminishing, while the mean distance re-

* See page 174.

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mains unchanged. In consequence of this, the mean disturbing force of the Sun is also diminishing, and therefore the Moon's gravity towards the Earth at any given distance is, on the whole, increasing. Also the area described by the Moon about the Earth is not affected by this alteration of the central force, whence it may be readily inferred that the Moon's mean distance from the Earth will be diminished in the same ratio as the force at a given distance is increased, and that the mean angular motion will be increased in double the same ratio."

In the *Mécanique Céleste*, the approximation to the value of the acceleration is confined to the principal term, which depends on the first power of the Sun's disturbing force.

In the theories of Damoiseau and Plana, a great extension is given to the theory of Laplace by taking into account the square and higher powers of the disturbing force, but the principle of the methods by which they determine the value of the acceleration, may be regarded as essentially the same as that of Laplace's method, which has just been explained.

Now, it will be observed that the reasoning employed in this method is founded on the supposition, that the area described in a given time by the Moon about the Earth undergoes no permanent alteration, or, in other words, that the tangential disturbing force produces no permanent effect. If we confine our attention to the first power of the Sun's disturbing force, as Laplace has done, this supposition holds good, but if we proceed to take into account the square and higher powers of the disturbing force, the same supposition is no longer strictly true.

In Mr. Adams' Memoir he endeavours to point out, in popular language, the manner in which the inequalities of the Moon's motion are modified by a gradual change of the mean central disturbing force, so as to give rise to a permanent alteration of the mean areal velocity of the Moon about the Earth.

As an example, he takes the inequality called the *variation*, which is the most direct effect of the disturbing force.

In the ordinary theory, the orbit of the Moon, as affected by this inequality only, would be symmetrical with respect to the line of conjunction with the Sun, and the areal velocity generated while the Moon was moving from quadrature to syzygy would be exactly destroyed while it was moving from syzygy to quadrature, so that no permanent alteration of areal velocity would be produced.

In reality, however, the magnitude of the disturbing force by which this inequality is caused depends in some degree on the excentricity of the Earth's orbit, and as this is continually diminishing, the central disturbing forces at equal angular distances on opposite sides of conjunction will not be exactly equal. Hence the orbit will no longer be symmetrically situated with respect to the line of conjunction. Now the

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change of areal velocity produced by the tangential force at any point depends partly on the value of the radius vector at that point, and consequently the effects of the tangential force before and after conjunction will no longer exactly balance each other.

The other inequalities of the Moon's motion are similarly modified, especially those which depend more directly on the excentricity of the Earth's orbit, so that each of them gives rise to an uncompensated change of the areal velocity.

The distortion in the form of the Moon's orbit, which is thus produced by the continual alteration in the excentricity of the Earth's orbit, is of the order of the Sun's disturbing force. Hence the alteration of the tangential disturbing force due to this distortion will be of the order of the square of the Sun's disturbing force. And, since this alteration of the tangential force is the only cause which produces a permanent change in the Moon's areal velocity, it follows that the rate of change of the mean areal velocity will also be of the order of the square of the Sun's disturbing force.

It is evident that this change of the Moon's mean areal velocity will give rise to a change of the same order in the amount of the lunar acceleration.

After giving this general reasoning in the introduction to his Memoir, Mr. Adams proceeds to the strict mathematical treatment of the problem.

For the sake of simplification he confines his attention to the terms which are independent of the excentricity and inclination of the Moon's orbit, and which do not involve any higher power of m than the fourth — m denoting, as usual, the ratio of the Sun's mean motion to that of the Moon.

He arrives at the conclusion that instead of the secular equation contained in Plana's expression for the true longitude in terms of the mean, namely,

$$-\left(\tfrac{3}{2}m^2-\tfrac{2187}{128}m^4\right)\int(e'^2-\mathrm{E}'^2)\,n\,dt$$

the following secular equation should be substituted

$$-\left(\tfrac{3}{2}m^2-\tfrac{7542}{128}m^4\right)\int(e'^2-\mathrm{E}'^2)\,n\,dt$$

The principal term of the correction to be applied to Plana's value of the secular acceleration is therefore

$$\tfrac{5355}{128}m^4\int(e'^2-\mathrm{E}'^2)\,n\,dt$$

And as the value of the integral

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$$\int (e'^2 - E'^2)\, n\, dt = -\,1270''\left(\frac{t}{100}\right)^2 \text{ nearly}$$

where t is expressed in years, the numerical value of this term is

$$-\,1''{\cdot}66\left(\frac{t}{100}\right)^2$$

Of course it might be expected that the succeeding terms of Plana's expression for the secular equation would also be materially changed.

It was not till 1856 that this important Memoir of Professor Adams' attracted attention, when M. Plana himself was induced by it to re-examine a portion of his *Théorie du Mouvement de la Lune*. In a paper published in April of that year, he admitted that his theory was imperfect, and he deduced Mr. Adams' result from his own equations. Soon afterwards, however, M. Plana retracted his admission of the correctness of Mr. Adams' result, and obtained another result, differing both from that which he had originally found, and also from that of Mr. Adams.

In 1857 Professor Hansen's valuable work, entitled *Tables de la Lune*, was published. In this work the coefficient $12''{\cdot}18$ was adopted for the secular acceleration of the Moon's mean motion.

M. Delaunay in 1859 undertook the investigation of this part of the lunar theory, and in the first instance carrying the calculation as far as the term involving m^4, he found exactly the same value for this term as Mr. Adams had done, namely $\frac{3771}{64}\, m^4$. This result was communicated to the Academy of Sciences on the 17th January, 1859. On being informed of this, Mr. Adams immediately published the values which he had some time previously obtained for the terms involving m^5, m^6, and m^7 (*Académie des Sciences de Paris*, 31 January, 1859, *Monthly Notices*, April 8, 1859*). (This paper contained an erroneous coefficient, which was subsequently corrected. It arose from writing $\frac{3372845}{10368}\, m^5$, instead of $\frac{3572845}{10368}\, m^5$, and was therefore simply a clerical error.) The result of Mr. Adams' new investigation was to reduce the coefficient of the lunar acceleration to $5''{\cdot}7$, or about half the value hitherto received. In a supplement to this paper† Mr. Adams communicated the values of the two principal terms in the expression of the lunar acceleration, which depend on the excentricity and inclination of the orbit. He found that the

* *Monthly Notices*, vol. xix., p. 207.

† Ibid. p. 208.

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term in e^3 increases the coefficient of the acceleration by $0''{\cdot}036$, while the term in γ^2 diminishes it by $0{\cdot}097$, so that this coefficient is ultimately reduced to $5''{\cdot}64$. I call attention here to a remark of Mr. Adams, which was expanded in a subsequent communication into a fuller anticipation, that another cause besides gravitation was concerned in producing the lunar acceleration. He says that he believes the value just given to be within one-tenth of a second of the true *theoretical* value of the coefficient of the secular acceleration, and adds, "Whether ancient observations admit of such a small value of the acceleration is a different question." M. Delaunay, on the 25th April, 1859, communicated to the *Académie des Sciences* the result of a more elaborate investigation, wherein he confirmed all the new terms of Prof. Adams just alluded to, and, by carrying the approximation to the eighth order, fixed the lunar acceleration at $6''{\cdot}11$ in a century.

It was after the publication of the foregoing results that the controversy commenced which occupied so much of the attention of physical astronomers. The propriety of introducing the new terms developed by Mr. Adams was called into question by M. de Pontécoulant (*Académie des Sciences*, 30 May, 1859), and, under date of the 28th of the same month, this distinguished mathematician communicated a note on the same subject to the then President of our Society (Rev. R. Main)*. In a letter from Prof. Hansen† to the Astronomer Royal, May 21, 1859, attention is called to those three values at which he had arrived at different periods, and which I have before enumerated (see pages 164 and 167). With the solicitude which he has invariably evinced for the interests of astronomy, Mr. Main undertook the examination of the question, and communicated to the Society‡ an elaborate paper "On the Present State of the Controversy respecting the amount of the Acceleration of the Moon's Mean Motion," wherein he clearly and fairly stated the bearings of the points under dispute, and thus narrowed the field of controversy—"It is incumbent on the opponents of Mr. Adams to show clearly that the tangential force, producing alterations of the areal velocity, can produce no effect on the secular acceleration." He at that early stage stated that "Adams and Delaunay seem to have right on their side." "But if we accept Mr. Adams's value of the coefficient, and the principles of his investigation be established beyond controversy, as in my own mind I have very little doubt they will be, this value is far too small to satisfy the ancient eclipses, and therefore some other cause (such as a resisting medium) totally different from the disturbing influences

* *Monthly Notices*, vol. xix., p. 235.
† Ibid. p. 236.
‡ Ibid p. 268.

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of the Sun and planets, must be resorted to, or we must hope, from a hint dropped by M. Delaunay at the end of his paper, that he has some means, at present kept out of sight, for laying the ghost he has helped to raise."

In the *Monthly Notices* for April 1860, vol. xx. p. 225, Mr. Adams published his "Reply to various objections" which have been brought against the theory of the secular variation of the Moon's mean motion.*

In this paper he calls attention to the fact that the question under consideration is a purely mathematical one, with the decision of which *observation* has nothing whatever to do.

If, as seems probable, ancient observations should show that the Secular Variation of the Moon's Mean Motion is different from that which, according to theory, would be produced by the known change of the excentricity of the Earth's orbit, it would be necessary to draw the conclusion that *the mean motion of the Moon is affected by some other cause or causes, besides the variation of the excentricity which has been taken into account.* Mr. Adams remarks that "this fact, if established, would be a most interesting one, and might put us on the traces of an important physical discovery."

It has been already mentioned, at the outset of these remarks, that the object of Mr. Adams' investigations was to find the effect on the Moon's motion of a slow, uniform change in the excentricity of the Earth's orbit, while the other elements of that orbit were supposed to remain constant.

Of course, in nature, the change of excentricity of the Earth's orbit cannot take place without being accompanied by changes in the other elements of the orbit, and in addition to the secular change of excentricity there will be other changes which are periodic.

If any part of the lunar acceleration could arise from such changes of the elements as those which have just been mentioned, of course this part would not be given by Mr. Adams' theory, which only professes to determine what amount of acceleration is due to the regularly progressive diminution in the excentricity of the Earth's orbit. If it should turn out to be true that the acceleration is partly due to such periodic changes in the elements of the Earth's orbit, this would in no way be inconsistent with Mr. Adams' result.

In the paper last cited, Mr. Adams points out that he has made *no assumption* in his Memoir of 1853 respecting the variability of the Moon's mean areal velocity. He *proves* mathematically that this areal velocity does vary and finds the amount of its variation, and the general reasoning given in the introduction is simply the translation, so to speak, of his analysis into ordinary language, in order to make the nature of his correction to Plana's theory more generally intelligible.

* See Appendix A.

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He remarks, too, that even if he had started with the assumption that the mean areal velocity was variable, no error could have been caused thereby, for if this areal velocity had been really constant, he would have simply found its variation equal to zero. In mathematical language, the terms *constant* and *variable* are not looked upon as exclusive of each other, but a *constant* is regarded as a particular case of a *variable* quantity.

Mr. Adams also states that in his investigations made subsequently to his Memoir of 1853, he employed a new method in which his results are obtained without taking into consideration the mean areal velocity at all.

It was suggested by M. Hansen, that the difference between his value of the secular acceleration and that obtained by M. Delaunay and Mr. Adams, might arise from a want of convergency in the series proceeding according to powers of m, by means of which they determine the coefficient of the acceleration.

In order to remove all possible objection, Mr. Adams calculated the value of that part of the coefficient of the acceleration which is independent of the excentricity and inclination of the Moon's orbit, by a method which does not require any expansion in powers of m, and the resulting coefficient exactly agreed with that which he had previously found by means of the series.

It would be needless to reproduce here the various arguments which were used to show that Mr. Adams was wrong in taking into consideration the variability of the areal velocity of the Moon, or to make any further reference to the objections brought against the principles on which he had conducted his investigations, because ultimately his results, and consequently those of M. Delaunay, were confirmed by the independent investigations of several mathematicians. For example, by the late Sir John Lubbock,* who used in this investigation formulæ which he had before employed in recalculating many of the inequalities of the Moon's motion. This distinguished mathematician, who contributed so greatly to the development of the lunar theory, arrived at precisely the same value for the much-contested term in m^4 as Mr. Adams had done in 1853.

Professor Donkin, in a communication read June 14th, 1861, and published in the *Monthly Notices*,† gave the results of an investigation of the coefficient of m^4, the calculation of which involves the whole mathematical question of the subject then under dispute. He used the same method of the variations of elements which had been employed by M. Delaunay; but as he

* *Mem. Ast. Soc.* vol. xxx. pp. 38–52, "On the Lunar Theory," read November 9, 1860.

† Vol. xxi. p. 221.

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had not had the opportunity of seeing what M. Delaunay had published, his result may be regarded as independent testimony of the correctness of Mr. Adams' value for that term.

Professor Cayley,* by following a totally different method from any used previously, also undertook the calculation of the coefficient of m^4, and completely confirmed Mr. Adams' value.

M. Delaunay had previously, by two entirely different methods, also arrived at the same value as Mr. Adams.

Baron Plana had, in 1856, as I have before stated, recognised the correctness of Mr. Adams' value for the coefficient of m^4; but subsequently, in a paper read before the Academy of Sciences of Turin, in June 1856, he retracted that admission, having arrived at a new result by a subsequent investigation. Ultimately, in 1860, after repeating his doubts as to the correctness of Mr. Adams' coefficient for m^4, he,† in a correspondence with Sir John Lubbock, admitted the correctness of Mr. Adams' value.

In these numerous confirmations of the correctness of the result of Professor Adams, the verifications were obtained by different methods, in which the *savants* who employed them did not start with the assumption either of a variability or of a non-variability of the areal velocity; and the calculations were consequently made without regard to the areal velocity at all, the variability or non-variability of the mean areal velocity being in fact part of the problem to be determined.

After the thorough investigation which the problem of the secular equation of the Moon's motion has received at the hands of so many distinguished mathematicians, there can no longer be any question as to the correctness of the value of the coefficient determined by Mr. Adams and M. Delaunay. But here comes a difficulty which, however, in no way casts any doubt on Mr. Adams' theory: it is this, that the coefficient $5''{\cdot}7$, or $6''{\cdot}11$, as M. Delaunay has made it by further developing the series, does not account for the amount of the secular acceleration as shown by observation. It is perfectly true, notwithstanding some uncertainty as to the actual date and place of the ancient solar eclipses, and also as to the correctness of the records of certain lunar eclipses, that Prof. Hansen's value of the secular acceleration, namely $12''{\cdot}18$, accords better with these phenomena than the smaller number $6''$.

Mr. Hartwig, in the *Astronomische Nachrichten*, No. 1241,‡ gives the results of his calculations of Nineteen Lunar Eclipses contained in the *Almagest*, by means of Hansen's Solar and Lunar Tables.

He comes to the conclusion that the results, on the whole, accord satisfactorily with Hansen's Tables.

* Abstract, *Monthly Notices*, vol. xxii. p. 32; Paper in full, ib. p. 173.
† See *Add. à la Conn. des Temps*, 1864, p. 56.
‡ 1860, p. 257. See Appendix B.

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In the *Berichte der K. Sächs. Gesellschaft zu Leipzig*, 15 April, 1863, is contained a short paper by Prof. Hansen, in which he states that as the value of the acceleration, as calculated by Adams and Delaunay, is considerably—nearly 6″—less than that employed in his Lunar Tables, such smaller value (as he had shown in the *Comptes Rendus*, t. l., 1860·0) cannot represent the ancient eclipses.* Assuming Adams and Delaunay's coefficient to be the correct acceleration due to the change in the excentricity of the Earth's orbit, the deficiency must be accounted for by other causes. That which most naturally presents itself is a change in the length of the sidereal day or period of the Earth's rotation; and he remarks that Laplace's demonstration of the invariability of the sidereal day depends on the very assumption that the acceleration is *wholly* accounted for by the change of the excentricity of the Earth's orbit, and consequently must in the present question be put aside.† He proceeds to show that the deficiency of 6″ in the acceleration would be accounted for by the almost infinitesimal increase of $0^s{\cdot}01197$ in the length of the sidereal day in the course of 2000 years.‡ He, however, goes on to say that it is remarkable that the value of the acceleration as obtained by himself should agree so well with observations without the assumption of any such supplemental cause.

In the second part of Hansen's *Darlegung der theoretischen Berechnung, &c.*, which has recently appeared, he has recalculated the secular variation of the mean longitude, and has found the coefficient 12″·557. It has been pointed out to me, however, that in his investigation he appears to have fallen into an error of the very same nature as that of Plana and Damoiseau to which I have already alluded. It is true that in the passage in Art. 219 Prof. Hansen professes to have already taken account of the terms to which Mr. Adams called the attention of astronomers, but notwithstanding this it would appear that he entirely neglects the non-periodic term which the terms referred to will introduce into the expression of the Sun's tangential disturbing force.

In fact, in the paragraph commencing at the foot of page 3 and ending at line 12, page 4, Prof. Hansen expressly states that his calculation of the secular acceleration proceeds on the assumption that $\mathfrak{A} = 0$, or in other words that this term has no secular variation.

Now since

$$\mathfrak{A} = -1 - \frac{h_0}{h} + 2\frac{h}{h_0} - \frac{3e_0}{1-e_0^2}\frac{h}{h_0}\{e\cos(\chi - n_0 y t - \varpi_0) - e_0\}\ddagger$$

* Abstract, *Monthly Notices*, vol. xxiii., p. 210.

† See the quotation from Laplace, page 164.

‡ Hansen's *Darlegung*, part i, art. 88, p. 332.

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this is equivalent to assuming that $\frac{h_0}{h}$ has no secular variation, or that the Moon's mean areal velocity has no secular variation, since the quantity which Hansen denotes by h is inversely proportional to the areal velocity.* Accordingly, in part ii. Art. 305, p. 339, the value found for $\Delta \frac{h_0}{h}$ contains no non-periodic term, and the terms which would have naturally led to the introduction of such non-periodic term are expressly neglected, because they would produce an effect on the element Ξ, whereas it is assumed that, in the calculation of the secular acceleration, this element may be put = 0. This, at all events, would appear to be implied by the passage near the beginning of Art. 305: "*Und lässt dabei die Glieder, die kein t ausserhalb des Cosinuszeichens haben, weg, die später nur in dem Element* Ξ *Wirkung äussern können, und daher zufolge der Annahme* $\Xi = 0$ *in Bezug auf die Säcularänderung der mittleren Länge hier wirkungslos bleiben.*" The only reason for this assumption that $\Xi = 0$, which Prof. Hansen mentions in the passage at page 4 of his 2d part above referred to, is the agreement between his value of the secular acceleration and that given by the ancient eclipses.† But it is evident that the secular part of Ξ should be determined by theoretical considerations alone.

It is a purely mathematical process which leads to the values of the quantities contained in Ξ, and it is by the substitution of these values in this element that it must be determined whether Ξ is equal to zero, or whether it has any other value. It is evident that observation cannot decide whether a mathematical process is correct or not, for the very question is, whether the whole of the effect shown by observation is due to the cause which has been taken into account in the mathematical theory, or whether part of it may be due to some other cause.

Indeed it now appears, from a letter which Professor Hansen has done me the honour to communicate to me, under date of Jan. 29 of this year, that some misapprehension has existed in respect of the interpretation which has been put upon his meaning hitherto, for he says, "I have never contested the ideas of Mr. Adams in regard to the secular equation of the

* Hansen's *Darlegung*, part i., p. 106.

† In Art. 298, p. 322 of the 2nd part of the *Darlegung*, M. Hansen refers to his second memoir "On the Perturbations of the Small Planets" for a proof that Ξ can contribute nothing to the secular variations; but it should be remarked that this proof only takes into account terms which are of the *second* order with respect to the disturbing forces, whereas the terms which Mr. Adams has added to the Moon's secular acceleration are of the *third* order with respect to the disturbing forces, since these terms are of the the order of the square of the Sun's disturbing force multiplied by the change of the excentricity of the Earth's orbit, and this change is itself of the order of the disturbing forces of the planets on the Earth.

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Moon, but I could not make use of the coefficient derived from his investigation, because it does not accord with observation. It is an inevitable condition which must be fulfilled, in calculating tables of the Moon or the planets, that they represent observations as far as possible. And if in any point the theory does not satisfy this condition, we must change the corresponding coefficient."* From this important explanation of Professor Hansen, it becomes all the more necessary to fix the dates and localities of ancient eclipses with the utmost attainable precision. For the attempts which have been made with more or less success in this direction we are under great obligation to Professor Hansen† himself, to the Astronomer Royal,‡ to Prof. Hansteen,§ and among the pioneers in this now increasingly important branch of inquiry, Mr. Bosanquet must be placed in the foremost rank.‖

Thus the value of the lunar acceleration due to the change in the excentricity of the Earth's orbit, which has been determined by Mr. Adams and M. Delaunay, may be regarded as established beyond all doubt, while at the same time it seems incontestable that a larger value is required in order to satisfy the ancient eclipses.

M. Delaunay has recently endeavoured to remove this difficulty by showing that there is a physical cause in operation adequate to explain the difference between the theoretical and the observed values of the acceleration. In a memoir read before the *Académie des Sciences*, on the 11th Dec. 1865,¶ he developes the following proposition:—The disturbing forces, to which are due the periodical oscillations of the surface of the seas, by their action on the liquid protuberances to which they give rise, tend to diminish the motion of the rotation of the Earth, and thus produce a sensible apparent acceleration in the mean motion of the Moon.—This communication is worthy of the earnest consideration of all physical astronomers; and although it is possible that immediate adherence may not be given to M. Delaunay's theory, it is sure to command the attention of those best qualified to discuss its merits.

That the tidal wave tends to cause a retardation of the Earth's rotation has been pointed out by Dr. J. R. Mayer, of Heilbronn, in his *Celestial Physics*, 1848;** and this action of

* See Appendix D.

† Hansen's *Darlegung*, 2nd part, pp. 386, 392, 398. See also Appendix C.

‡ *Phil. Trans.* 1853, *Mem. R. A. Soc.* 1858.

§ *Ergänzungs-Heft* to the *Astron. Nach.*, published in 1849.

‖ See Appendix E.

¶ *Comptes Rendus*, tome lxi. Séance du 11 Dec., 1865:—see also Abstract *Month. Not.* vol. xxvi. p. 85.

** *Beiträge zur Dynamik des Himmels, in Populärer Darstellung.* Heilbronn, 1848. Translated by Dr. H. Debus, *Phil. Mag.*, fourth series, 1863, vol. xxv., p. 403.

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the Moon has been made familiar to the English public by Dr. Tyndall's *Lectures on Heat.*

It is not my intention to enter upon the merits of the Memoir of M. Delaunay in this address, but I deem it right to point out that while Mayer and others have seen that the tidal wave must act as a sort of brake tending to retard the rotation of the Earth, this illustrious French mathematician is the first who has attempted to prove that the effect is an appreciable quantity, *and such as could account for that part of the lunar acceleration which is not produced by the secular change in the excentricity of the Earth's orbit.* This is a grand result, and if confirmed, it must be ranked amongst the most important contributions to astronomical physics.

The secular change in the excentricity of the Earth's orbit, although of long period, is, after all, periodic, and hence that part of the acceleration of the Moon's mean motion which is dependent upon it is also periodic. This is not the case in respect of that part (the apparent acceleration) which arises from the increase of the time of the Earth's rotation, for that part will go on accumulating for ever; countless ages will, however, pass away before the length of the day will be so altered as to change in an appreciable degree the conditions of things upon the Earth. But the exact calculation of the effect of the tides in retarding the Earth's rotation is fraught with such great difficulties that it is most probable that its amount will be best obtained by comparing the whole observed amount of the lunar acceleration with that part of it which is theoretically computed from the reflex action of the planets, the primary effect of which is to change the excentricity of the Earth's orbit.*

We thus see that the re-examination of Laplace's theory of the Secular Equation by Mr. Adams has indirectly led to a more formal investigation of how far the effect of the Sun and Moon on the protuberance of the great tidal wave may produce a retardation of the Earth's rotation equivalent to the difference between Hansen's coefficient $12''{\cdot}557$, and Adams' and Delaunay's value $6''{\cdot}11$.

It should be observed that the question of what part of the acceleration is real and due to the Sun's action, and what part is merely apparent and due to a change in the time of the Earth's rotation, is by no means without practical importance.

For the part of the acceleration due to the first of the above-mentioned causes is peculiar to the Moon, whereas the part due to the other cause affects the places of all celestial bodies in proportion to their angular velocities. Thus, if the change in the time of the Earth's rotation produces an

* See Appendix F.

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apparent acceleration of 6″ in a century in the motion of the Moon, the same cause must produce an acceleration of nearly 0″·5 in a century in the motion of the Earth (or in the apparent motion of the Sun), and nearly 2″ in a century in the heliocentric motion of *Mercury*.

I have now to speak of very important investigations of Professor Adams', in respect of the Moon's Parallax. Though I do not claim for these researches the same originality of ideas and method which characterised the memoir on the Secular Acceleration of the Moon's Motion, yet they are the representatives of a vast amount of labour, and are in the highest degree creditable to the sagacity and industry of the author.

It is needless for me to dilate on the importance of an accurate knowledge of the lunar parallax, for no observation of the Moon's place can be compared with the theoretical place as derived from the tables, without a previous reduction, wherein the question of the amount of parallax has first to be taken into consideration. Mr. Adams, in examining Burckhardt's Tables, found that those relating to the lunar parallax were defective.

The principal results of Mr. Adams' investigations respecting the Moon's parallax are contained in a paper "On new Tables of the Moon's Parallax, to be substituted for those of Burckhardt," which was published in the Supplement to the *Nautical Almanac* for 1856,* and in another paper, "On the Corrections to be applied to Burckhardt's and Plana's Parallax of the Moon, expressed in terms of the Mean arguments," which appeared in the *Monthly Notices of the R. A. S.*, vol. xiii. p. 262.

Mr. Adams' attention was called to the discrepancies between the values of the Moon's parallax, as calculated from the Tables of Burckhardt and of Damoiseau, by reading Mr. Henderson's paper on the Constant of Lunar Parallax in the tenth vol. of the *Memoirs of the R. A. S.* Mr. Henderson's determination of this constant is based on thirty-four observations of his own, made at the Cape of Good Hope, combined with corresponding observations at Greenwich and Cambridge. The values of the parallax deduced from the observations are compared with those calculated by means of the Tables, both of Burckhardt and of Damoiseau.

Now it is remarkable that the values of the constant of parallax thus found differ by 1″·3, according as one set of Tables or the other is employed in the comparison. Not knowing which of these values to prefer, Mr. Henderson adopts the mean of the two for his final result. A little consideration, however, will suffice to show the unsatisfactory

* Mr. Adams' Tables of the Moon's Parallax have been reprinted in the *Addit. à la Conn. des Temps* for 1856, and also in the *Berliner Jahrbach* for the same year.

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nature of this mode of procedure. It is to be remarked that the only part of the investigation which is dependent on the Tables consists in the reduction from the actual values of the parallax at the times of observation, to the constant or the mean value of the parallax. Now it is plain that so large a difference as 1″·3 in the mean amount of these reductions corresponding to the thirty-four observations concerned, can only arise from intolerable errors in the periodic terms of the parallax given by one of the two sets of Tables. The two results must, therefore, be regarded as contradictory of each other, and must not be combined together. One of the results may be good, but if so, it is sure to be vitiated by combination with the other which must be decidedly erroneous.

Damoiseau's Tables give the parallax at once in the form in which it is furnished by theory, but the expression of the parallax which is used in Burckhardt's Tables is adapted to his peculiar form of the arguments, and requires transformation in order to be compared with the former. When this transformation had been effected, Mr. Adams found that several of the minor equations of the parallax deduced from Burckhardt differed essentially from their theoretical values given by Damoiseau. With regard to the parallax, Burckhardt professes to have followed the theory of Laplace, but as this agrees very closely with that of Damoiseau, it is clear that errors must have crept into Burckhardt's transformation of Laplace's formula. These errors appear to have arisen in the following manner.

In the formation of Burckhardt's arguments of evection and variation, the mean longitude of the Sun is employed. Now, four of the errors in the coefficients of the minor equations may be accounted for, by supposing him to have erroneously employed the true instead of the mean longitude of the Sun in forming the above-mentioned arguments. In another of the minor equations, the coefficient is taken with a wrong sign, and in another a wrong argument is employed.

On further inquiry, Mr. Adams discovered that the difference between Burckhardt's equations of parallax and those of Burg and Damoiseau had been long since remarked by Clausen in a valuable comparative analysis of the three sets of lunar tables given in the 17th volume of the *Astronomische Nachrichten*, but this remark seems to have excited no attention whatever.

After examining Burckhardt's Tables of Parallax, Mr. Adams was naturally led to scrutinize more closely the results of the theories of Damoiseau, Plana, and M. de Pontécoulant, in relation to the same subject. Although the differences between the results of the several theories were, in general, small when compared with the errors of Burckhardt, still they were greater than we had a right to expect would have been the case, considering the close agreement which existed

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between the several theories with reference to the equations of longitude.

On examining the processes which had been employed by the several authors, he found that the numerical values of the coefficients of the equations of parallax assigned by them by no means fairly represented the most accurate values of those coefficients which the several theories were capable of giving.

In the theories of Damoiseau and Plana, the expression for the reciprocal of the projection of the Moon's radius vector upon the plane of the ecliptic in terms of the Moon's true longitude, is required in order to find the relation between that longitude and the time, and consequently the utmost care has been taken to obtain that expression with accuracy.

In the subsequent operations and transformations necessary in order to deduce the expression for the parallax, first, in terms of the Moon's true longitude, and finally, in terms of the time, the same care has by no means been employed. In Damoiseau's theory, the coefficients of the expression for the reciprocal of the projected radius vector are only given numerically, and the quantities neglected in the subsequent transformations are not very important, although they are still sensible. In Plana's theory, the coefficients of the same expression are given in their analytical form, and the approximations are carried to the seventh, or even to the eighth order of small quantities, in cases where this degree of accuracy is requisite. In the transformations for finding the parallax, however, Plana neglects all quantities which are of higher orders than the fifth, and consequently many of his coefficients are very sensibly in error.

In M. de Pontécoulant's theory, the time is taken as the independent variable, and the analytical expression for the parallax in terms of the time is obtained immediately, and is developed to as great an extent as the corresponding expression for the longitude. In the conversion of his formulæ into numbers, he, however, neglects all the terms beyond the fifth order, and therefore many of his coefficients, like Plana's, are sensibly erroneous.

Mr. Adams has endeavoured to supply the defects and omissions in the several theories which have been above pointed out. He has transformed anew Damoiseau's and Plana's expressions for the reciprocal of the projected radius vector, so as to obtain the expression for the parallax in terms of the time, with all the accuracy of which the respective theories admit, and he has also converted into numbers the terms of M. de Pontécoulant's expression for the parallax which have been neglected by M. de Pontécoulant himself.

In the course of these operations, Mr. Adams has succeeded in detecting and tracing back to their sources a considerable number of errors, especially in Plana's work, and has thus

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been able to remove many of the discrepancies between the results of Baron Plana and M. de Pontécoulant.

When the values of the parallax given by the several theories had been corrected in the manner above described, the agreement between them was found to be most satisfactory. The difference between the separate values of each coefficient of parallax and the mean of all, rarely amounted to a hundredth part of a second.

As an example of the differences between the values of the coefficients of parallax originally given by the several authors, we may take the coefficient of the principal term in the variation inequality, the argument of which, expressed in Damoiseau's notation, is $2t$.

The several values given for this coefficient are the following :—

Damoiseau	.. ..	28·54
Plana ..	.. ..	27·59
Pontécoulant	.. ..	27·475

whereas all the theories, when corrected, agree in giving the value

$$28''\cdot23$$

In one of Plana's coefficients the error amounts to not less than $1''\cdot9$, but this simply arises from the principal term in the analytical expression of this coefficient having been inadvertently taken with a wrong sign, in the conversion of the formula into numbers.

Mr. Adams has taken particular pains with the theoretical determination of the Constant of Parallax, and with regard to this the agreement of the several theories is perfect. From Dr. Peters' value of the Constant of Nutation, it may be deduced that the ratio of the Earth's mass to that of the Moon is as 81·5 to 1 very nearly. Employing this ratio, together with the dimensions of the Earth according to Bessel, and the length of the seconds' pendulum in latitude $35\frac{1}{4}°$, deduced from Mr. Baily's Report on Foster's Pendulum Experiments, he finds the value of the Constant of Parallax to be $3422''\cdot325$.

This result agrees admirably with that found by Mr. Henderson, in the paper cited above, when the observations are reduced by means of Damoiseau's Tables.

In the seventeenth volume of the *Astronomische Nachrichten*, M. Hansen gave the value of the parallax which he had at that time obtained by means of his new method of treating the lunar theory. Mr. Adams transformed this expression with great care, so as to compare it with the results of the former theories. The agreement is in general very close; the difference between the values of the co-efficients seldom exceeding a hundredth part of a second.

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There are only two instances in which the difference is much greater than this quantity. One of these cases is that of the Constant of Parallax, the value of which, given by M. Hansen's theory, is 0″·06 less than the corresponding value found from the same fundamental data by the other theories. The second case is that of the term whose argument expressed in Damoiseau's notation is $t+z$, the co-efficient being 0″·146 according to Damoiseau and Plana, 0″·140 according to Pontécoulant, and 0″·181 according to Hansen.

In both these cases Mr. Adams finds that Hansen's definitive values of the co-efficients as given in his Lunar Tables differ sensibly from those given in the paper above referred to, and agree closely with those which result from the former theories.

In the formulæ given in Mr. Adams' published papers on Parallax, referred to at the beginning of this notice, quantities less than 0″·05 have been neglected, except in cases where they can be included in the same table with larger terms. In Mr. Adams' computations, however, quantities far smaller than this have been taken into account, and an additional place of decimals has been employed, beyond that retained in the results.

The total error in the periodic terms of Burckhardt's Tables of Parallax may amount to nearly 6″, and independently of this his constant of parallax requires an increase of nearly 2″.

It is unfortunate that so erroneous a value of the Parallax, as that of Burckhardt, should have been so long employed in the *Nautical Almanac*, and consequently should have been used in the reduction of such a vast number of lunar observations.

In the Appendix to the *Nautical Almanac* for 1856, which contains Mr. Adams' Tables of the Parallax, are also given tables of corrections to be applied to the values of the Parallax given in the *Nautical Almanac*, for every day in each of the years from 1840 to 1855 inclusive. From 1856 to 1861 inclusive, the values of the Parallax given in the *Nautical Almanac* were computed by means of his tables. Subsequently to that time Hansen's Tables have been employed.

In Mr. Airy's "Reductions of the Greenwich Lunar Observations," from 1750 to 1830, Plana's formula for the Parallax has been employed. We have already seen that this formula requires corrections which are by no means insensible.

I could quote largely from my friend and a former occupier of this Chair, the Rev. R. Main, in elucidation of the vast amount of trouble which has been occasioned by the artificial arguments which had been devised by Burckhardt in the construction of his tables, but Mr. Main's lucid explanation of the claims of that fine old veteran Hansen to your gratitude, are so vivid in your recollection that it is needless for me to do so.

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The address which he delivered in 1860 contains so much of interest that it is worthy of re-perusal now that we are dealing with the utmost refinements of the lunar theory. My own experience has led to the opinion, often expressed, that a large portion of time is taken up in the correction of errors, and this remark most certainly applies to practical astronomy; most grateful ought we, therefore, to be to those men who, with zeal and untiring industry, occupy themselves in labours such as those to which I have called attention, and who by their sagacity have saved us from the uncertainty and useless toil invariably entailed by the use of erroneous tables.

Besides the works before mentioned, there are several investigations by Mr. Adams, the results of which have been published without the processes which have led to them; and it will not be considered out of place to direct attention to these works, although they do not constitute any part of the grounds on which the award of the Medal was made. One of these is an investigation of the relative positions in space of the two heads of Biela's Comet, as deduced by means of a method of his own from the apparent relative positions observed at Cambridge. This investigation showed that the relative motion during a month might be fully accounted for without supposing any sensible mutual action between the two heads. The periodic time of the smaller head was found to be 8·48 days longer than the periodic time of the larger. The results of this interesting investigation were published in the *Monthly Notices*, vol. vii. p. 83 (April 1846).

Among the works of Mr. Adams I find in the *Nautical Almanac* for 1851, a correction to Bouvard's Table XLII., of *Saturn*. The re-examination of the theory of this planet was suggested by the discordances between the computed and observed places exhibited in the *Greenwich Planetary Reductions*. No higher testimony to the practical value of this investigation is needed than to state that the results are now employed by Mr. Hind in the computation of the places of *Saturn* for the *Nautical Almanac*.

Another investigation involving considerable labour is that relating to the mass of *Uranus*, which will be found in the *Monthly Notices*, vol. ix. p. 159. Two values of this mass had been previously given, differing widely from each other. Bouvard, from the action of *Uranus* on *Saturn*, found the mass to be $\frac{1}{17918}$ of the Sun's mass; while more recently, from observations of the satellites, Lamont obtained the value $\frac{1}{24605}$. From a careful reduction of Mr. Lassell's observations of the fourth satellite (which are more to be depended on for this purpose than those of the second) Mr. Adams obtained the value $\frac{1}{20897}$, which is almost exactly a mean between the results of Bouvard and Lamont. He also reduced all Sir Wm. Herschel's measures of distance of the satellites, given in his paper in the *Phil. Trans.* 1815, and obtained from the ob-

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servations of the fourth satellite the quantity $\frac{1}{2\,1165}$, which agrees closely with that found from Mr. Lassell's observations. Mr. Adams arrived at the conclusion that $\frac{1}{2\,1000}$ is probably much nearer the true value of the mass than either of the values that had been previously given, and that it may be employed, provisionally at least, in the theory of *Neptune*, without risk of any considerable error.

In Admiral Smyth's *Ædes Hartwellianæ*, p. 341, and also in his *Speculum Hartwellianum*, p. 364, are published Mr. Adams' results respecting the orbit of the double star *γ Virginis*. The orbit given by Sir John Herschel, in the results of his *Cape Observations*, was taken as the basis of the calculations; and equations of condition for the correction of the elements were formed, by comparing certain selected angles of position deduced from observation with the values calculated by means of Sir John Herschel's elements. These equations of condition were formed by a method of Mr. Adams' own, which appears to be more simple than any other before used.

Laplace has said, that every difficulty which has arisen in explaining the inequalities of planetary movements has ultimately served to establish on a firmer basis that most brilliant discovery of Newton,—the law of Universal Gravitation. Among those ardent and illustrious mathematicians, who have contributed towards the clearing away of these difficulties, there is none who stands higher than the recipient of the Medal this day, whose name is and ever will be associated with that grand investigation of the perturbations of *Uranus*, by an unknown planet, with which he began his career. It does not come within the purpose of my present address to enter upon the connexion of Professor Adams with the discovery of the planet *Neptune;* it would suffice to say that those most competent to judge of the merits of this celebrated investigation have always been greatly impressed by the power and comprehensiveness with which, from beginning to end, he grasps all the bearings of this difficult subject, as well as by the clearness, directness of aim and precision, which characterize his whole treatise:—but I cannot refrain from recalling those memorable words uttered by Sir John Herschel, who, in speaking of Leverrier and Adams in connexion with that discovery, declared theirs to be "names which Genius and Destiny had joined," to "be pronounced together so long as language shall celebrate the triumphs of science in her sublimest walks."

And now, Professor Adams, it only remains for me to congratulate you most heartily on the full confirmation which your theory of the Secular Acceleration of the Moon's mean motion has received at the hands of so many distinguished analysts, and in presenting you with this Medal to express a hope that your health may long permit you to exercise that great intellect with which you have been endowed, and which you have so highly and so successfully cultivated.

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Appendix A.

(See *Monthly Notices*, vol. xx. commencing at p. 230.)

In Mr. Adams' "Reply to Various Objections" he points out that the error of M. Plana's original value of the secular acceleration, given in his "*Théorie du Mouvement de la Lune*," arises from his assumption that one of the so-called constant quantities introduced by integration is absolutely constant, whereas it is really subject to a secular variation. He also shows that the value of the acceleration given by M. de Pontécoulant in the *Comptes Rendus* of April 9, 1860, differing widely as it does from that of M. Plana, is vitiated by an assumption of exactly the same kind with respect to another constant introduced by integration.

In M. Plana's theory a constant is introduced which is denoted by h^2.

In M. de Pontécoulant's theory two constants are introduced, which are denoted by k and $\frac{1}{a}$ respectively.

These constants are not independent, but are connected with each other and with the Moon's mean motion n by the following relations:—

$$h = n^{-\frac{1}{3}} \left\{ 1 - \frac{1}{3}m^2 + \frac{719}{576}m^4 + e'^2\left[-\frac{1}{2}m^2 + \frac{2635}{384}m^4 \right] \right\}$$

$$k = n^{-\frac{1}{3}} \left\{ 1 - \frac{1}{3}m^2 + \frac{11}{144}m^4 + e'^2\left[-\frac{1}{2}m^2 - \frac{185}{96}m^4 \right] \right\}$$

$$\frac{1}{a} = n^{\frac{2}{3}} \left\{ 1 + \frac{2}{3}m^2 - \frac{1253}{288}m^4 + e'^2\left[m^2 - \frac{5593}{192}m^4 \right] \right\}$$

in which formulæ the sum of the masses of the Earth and Moon is supposed to be unity, the excentricity and inclination of the Moon's orbit are neglected, and powers of m above the fourth are omitted.

By differentiating these relations and observing that $\frac{dm}{m\,dt} = -\frac{dn}{n\,dt}$ since $m\,n$, which is the Sun's mean motion, is constant, we may obtain,

$$\frac{dn}{n\,dt} = -3\frac{dh}{h\,dt} + \frac{d(e'^2)}{dt}\left\{ -\frac{3}{2}m^2 + \frac{2187}{128}m^4 \right\}$$

$$\frac{dn}{n\,dt} = -3\frac{dk}{k\,dt} + \frac{d(e'^2)}{dt}\left\{ -\frac{3}{2}m^2 - \frac{297}{32}m^4 \right\}$$

$$\frac{dn}{n\,dt} = -\frac{3}{2}\frac{da}{a\,dt} + \frac{d(e'^2)}{dt}\left\{ -\frac{3}{2}m^2 + \frac{5337}{128}m^4 \right\}$$

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If $\frac{d\mathrm{h}}{\mathrm{h}\,dt}$ be neglected, or h be supposed to be constant, we obtain the value of $\frac{dn}{n\,dt}$ found in M. Plana's theory.

If $\frac{da}{a\,dt}$ be neglected, or a be supposed to be constant, we obtain the value of $\frac{dn}{n\,dt}$ found by M. de Pontécoulant, in his paper in the *Comptes Rendus* above referred to.

If $\frac{dh}{h\,dt}$ be neglected, we obtain the value of $\frac{dn}{n\,dt}$, which would have been found by M. de Pontécoulant, if he had supposed the quantity h to be absolutely constant instead of the quantity a.

The several values of $\frac{dn}{n\,dt}$ thus obtained are evidently contradictory, and the reason is, that the quanties h, h, and a, are really variable, and therefore $\frac{d\mathrm{h}}{\mathrm{h}\,dt}$, $\frac{dh}{h\,dt}$, and $\frac{da}{a\,dt}$, cannot be neglected. If the values of these differential coefficients be found in the way which Mr. Adams points out, and then substituted in the above equations, we find from all alike the value

$$\frac{dn}{n\,dt} = \frac{d\,(e'^2)}{dt}\left\{-\frac{3}{2}m^2 + \frac{3771}{64}m^4\right\}$$

which is the result obtained by Mr. Adams.

Damoiseau's value of the acceleration is not expressed in an analytical form, but his method of obtaining it is exactly equivalent to that of M. Plana.

M. Hansen, as is well known, does not express the lunar inequalities in terms of m, but, from the remarks which have been already made in this Address respecting his method of finding the acceleration, it appears probable that his process is equivalent to neglecting $\frac{dh}{h\,dt}$ or assuming that the quantity h is absolutely constant.

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Appendix B.

For the following tabular view of the results of Mr. Hartwig I am indebted to Mr. Marth, who remarks that Nos. 15 and 19 are favourable to Mr. Adams's acceleration, but that all the others are opposed to it. The prevailing sign of the differences between the recorded and computed times appears to favour a diminution of the coefficient for the secular acceleration employed in the tables of about 2″. The necessity however of rendering eclipse No. 8 physically possible (assuming that there is no error in the date) seems strongly opposed to such diminution.

No.	Date.	Place.	Greatest Phase. Recorded.	Cal.	Recorded Time.	Cal. Time.	Diff.
						h m	
1	−720 March 19	Babylon	total	1·62	Beg. a good hour after moonrise (which occurred towards 6^h).	7 8	
2	−719 March 8	,,	$\frac{1}{4}$	0·21	Middle uncertain, about midnight.	11 13	
3	−719 Sept. 1	,,	more than $\frac{1}{2}$	0·57	Beg. after moonrise (towards $6\frac{1}{2}^h$).	6 14	
4	−620 April 21	,,	$\frac{1}{4}$	0·07	Beg. $16^h\ 37^m$	15 56	-41^m
5	−522 July 16	,,	$\frac{1}{2}$	0·45	Mid. uncertain, an hour before midnight.	11 2	
6	−501 Nov. 19	,,	$\frac{1}{4}$	0·11	Mid. uncertain, about $11^h\ 20^m$.	11 24	
7	−490 April 25	,,	$\frac{1}{8}$	0·02	Mid. uncertain, about $11^h\ 28^m$.	10 14	
	−424 Oct. 9	Athens	total	1·34			
	−412 Aug. 27	Syracuse	,,	1·16			
	−405 April 15	Athens	,,	1·34			
8	−382 Dec. 22	Babylon	...	...	Beg. while half an hour of the night was yet left ($18^h\ 31^m$).	18 53	+22
					h m		
9	−381 June 18	,,	...	...	Beg. 7 47	25	−22
10	−381 Dec. 12	,,	total	1·42	Beg. 9 36	8 55	−41
11	−200 Sept. 22	Alexandria	...	...	End. 8 26	7 57	−29
12	−199 March 19	,,	total	1·44	Beg. 11 28	10 54	−34
13	−199 Sept. 11	,,	total	1·65	Beg. 12 36	12 15	−21
					Mid. 14 11	14 6	− 5
14	−173 April 30	,,	$\frac{7}{12}$	0·67	Beg. 12 49	12 8	−41
					Mid. 15 32	14 50	−42
15	−140 Jan. 27	Rhodes	$\frac{1}{4}$	0·29	Beg. 9 56	8 41	−75
16	125 April 5	Alexandria	$\frac{1}{8}$	0·11	Mid. uncertain, about $8^h\ 26^m$.	8 39	
17	133 May 6	,,	total	1·05	Mid. 11 8	10 41	−27
18	134 Oct. 20	,,	$\frac{5}{8}$	0·88	Mid. 10 47	10 37	−10
19	136 March 5	,,	$\frac{1}{8}$	0·43	Mid. 16 13	15 29	−44

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Appendix C.

I am indebted to Mr. Marth for the following notes referring to Hansen's Calculations of old Solar Eclipses.

The value of the acceleration of mean longitude employed in Hansen's Lunar Tables is 12″·180. His further researches (vide *Darlegung der theoretischen Berechnung*, &c., 2nd part, page 374) have led him to increase this value by 0″·377, and he has found, by extending his researches, corrections of the tabular retardation of the perigee and of the acceleration of the node.

	Hansen's Lunar Tables. Final Values.		Diff.
Secular Variation of Mean Longitude	$+12''\cdot180\,t^2$	$+12''\cdot557\,t^2$	+0·377
,, Perigee	$-37\cdot255\,t^2$	$-38\cdot577\,t^2$	−1·322
,, Node	$+\ 7\cdot068\,t^2$	$+\ 6\cdot623\,t^2$	−0·445

The tracks of the old total Solar Eclipses (arranged in chronological order) are altered by these corrections in the following manner:—

1. Eclipse of Thales, −584 May 28.
   The zone of totality is thrown 1° 7′ towards the north, and covers the greater part of the whole region within which it may be fairly assumed that the battle between the Lydians and Medes must have taken place. It covers, also, the Hellespont, and satisfies the remark of Theon (in his commentary on Ptolemy's *Almagest*), who speaks of an eclipse which was total in the Hellespont and its neighbourhood, while at Alexandria the greatest phase was at most $\frac{1}{2}$ths of the Sun's diameter. The northern limit of the zone of totality approaches also to Boghaskoei, lat. 40° 2′ N., long. 34° 21′ E., in the neighbourhood of which monuments cut in the rocks have been found, appearing to refer to the war between the Lydians and Medes, to the solar eclipse, and to the succeeding events.

2. Eclipse of Larissa, −556 May 19.
   The central track given by the tables passes 1′ north of Larissa. Hansen's final values throw the track 8′ more north, and the eclipse remains total.

3. Eclipse of Ennius, mentioned by Cicero, −399 June 21.
   The tables make the eclipse total for Rome, 5^m before sunset. Hansen's corrections give the greatest phase 0·987 (Sun's diameter = 1) 12^m before sunset, which seems quite compatible with the record: "Anno ccc quinquagesimo fere post Romam conditam Nonis Juniis soli luna obstitit et nox." (*Cic. De Republica*, I. 16.)

4. Eclipse of Agathocles, −309 August 14.
   The south-east point of Sicily, Cape Passaro, is about 45′ distant from the northern limit of the zone of totality as given by the tables. Hansen's corrections increase the distance to about 51′. In the uncertainty of Agathocles' real position at the time of the eclipse, we are left to conjecture how near he may have been to the zone of totality.

5. Eclipse of Stiklastad, 1030 August 31.
   The shortest distance of Stiklastad from the central track, as given by the tables, is 1° 20′, and from the northern limit of the zone of totality 1° 9′ (or, in case the ellipticity of the Earth is assumed to be $\frac{1}{297}$ instead of $\frac{1}{300}$, about 2′ less). Hansen's corrections reduce these distances to

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1° 11′ and 59′, and increase the greatest phase at Stiklastad from 0·985 to 0·988.

The corrected tabular values represent also—

6. The Eclipse of Rome, 1567, April 9, giving 1″·1 as the greatest breadth of the uncovered part of the Sun, as seen from the Collegium Romanum. Keppler's record—"Clavius in commentario in Sacroboscum recenset, anno 1567, Apr. 9, Romæ Solem non totum defecisse, sed relictum fuisse exilem quendam circulum lucentem circumcirca"—might indeed suggest an annular eclipse; it was however total in the neighbourhood of Rome, though only for a very short time.

If the retrograde motion of the node is less rapid by 12″ in a century than that of the Tables, an assumption which would be compatible with Bradley's observations, it would satisfy the circumstances of some of the old eclipses perhaps still better. The track of the eclipse of Thales would go still farther north; the eclipse of Larissa would remain total, though Larissa would now be near the northern limit; the eclipse of Ennius would have a greatest phase of 0·979, 11 minutes before sunset; the track of the eclipse of Agathocles would be thrown farther north, so that the northern limit of the zone of totality would pass at a distance of 28′ from Cape Passaro. Stiklastad would approach within 40′ to the northern limit of totality.

On the other hand, a diminution of Hansen's tabular acceleration of mean longitude by 6″ would not satisfy any of the old records (supposing that the years are correct). Accepting his final values of the secular variations of the perigee and node, the track of the eclipse of −584 (Thales) would be thrown more than 9° southwards, and would not reach the regions of the battle before sunset; the greatest phase of the eclipse of −556 would not exceed $\frac{2}{3}$ of the Sun's diameter at Larissa, and that at sunset; the eclipse of Ennius would not be at all visible at Rome, as it would happen after sunset; the southern boundary of totality of the eclipse of −309 (Agathocles) would be thrown to about 40′ north of Syracuse; the track of the eclipse of Stiklastad would be thrown 1° 18′ more south.

Appendix D.

At my request Professor Hansen has been so good as to communicate to me the following explanation of his reasons for using the larger coefficient of the lunar acceleration in his Lunar Tables:—

"I have never disputed the correctness of the Theory of the Secular Equation of the Moon's Longitude, such as Mr. Adams was the first to propound; but I am not satisfied with the development of the divisors into series, of which he has made use, as several other geometers have also done. As the coefficient which results from Mr. Adams' theory does not accord with observations, it could not be employed for the Lunar Tables: for in the construction of tables, either planetary or lunar, the first condition to be fulfilled, is to construct them in such manner that they represent observations as closely as possible, for without this they would be of no practical value, and therefore useless.

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"Mr. Adams' theory came too late to permit of my making use of it in my works; and it was well that it so happened, for I had already found by my own theory a coefficient which represents ancient observations as closely as could be desired; which is not the case with Mr. Adams' coefficient.

"As my two Memoirs, entitled *Darlegung*, &c. were destined to the development of the calculations by which I had arrived at the coefficients used in my Lunar Tables, I could not therein employ the theory of Mr. Adams; and in the introduction to the Second Memoir, p. 4, I have explained myself on this point, adding that ulterior researches on this subject were to be reserved for a special memoir. Subsequently I endeavoured to account for the fact that Mr. Adams' coefficient did not accord with observation, and I found that an extremely small retardation in the rotation of the Earth on its axis sufficed to explain it; this was published in the *Berichte der K. Sächs. Gesellschaft zu Leipzig* for 1863, in the note to which you have drawn attention. Already M. Mayer, of Heilbronn, in his *Beiträge zur Dynamik des Himmels, etc.*, a work with which at the time I was unacquainted, had directed the attention of geometers to the Tidal Wave as capable of causing such a retardation of the Earth's rotation; and recently M. Delaunay has submitted this point to analysis.

"In the actual position of the question it may be concluded from all these researches, that the acceleration of the Moon's motion depends on two causes, that is to say, the decrease of the excentricity of the terrestrial orbit and a retardation of the rotation of the Earth on its axis, and that it is the combined effect of these two causes which produces the amount of acceleration which observations show.

"It is, however, very remarkable, that under all these circumstances, the hypothesis that $\mathfrak{M} = 0$, in regard to the acceleration, leads to a value for the coefficient in question which represents the old eclipses as well as can be desired, and which for this reason ought to be considered as the true value of the coefficient for the secular equation of the mean longitude of the Moon.

"*Gotha*, 1866, *Feb.* 10."

"P. A. Hansen.

Appendix E.

Mr. Bosanquet deserves great credit for the zeal he has evinced in promoting the investigation of certain ancient eclipses. It appears that he was the first to call the attention of Astronomers, and more particularly that of Mr. Airy, to historical facts in connexion with the expedition of Agathocles, which it was necessary to take into account in interpreting that eclipse. Moreover, Mr. Bosanquet urged upon Mr. Airy's attention facts which induced him to search out and identify the important eclipse of Larissa; and he has brought forward evidence tending to show that the appearance occurring to Hezekiah* is connected with the solar eclipse which took place −688, Jan 11, when, he argues, that central conjunction took place about 20 minutes before noon, and that the eclipse was to the extent of about two-thirds of the Sun's diameter.

---

* "And Isaiah the prophet cried unto the Lord, and he brought the shadow ten degrees backward, by which it had gone down in the dial of Ahaz."—2 Kings, xx. 11.

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Prof. Hansteen was the first to bring under notice the eclipse of Stiklastad, "which," says the Astronomer Royal, "in combination with that of Larissa, appears likely to throw light upon the correctness of the lunar tables."

The interest of Mr. Airy in these ancient eclipses is naturally concentrated upon the data they afford for determining elements connected with the lunar motions. A perusal of his investigations published in the *Philosophical Transactions* in 1853, and in the *Memoirs of the Royal Astronomical Society* in 1858, will repay all who take an interest in the history of the development of the lunar theory. Although the main object of the papers is the fixing of the time and place of the phenomena under discussion, with a view to the correction of lunar elements, yet certain matters of historical and geographical interest are not neglected. Among these may be cited the fixing of certain limits within which the battle occurred between the Lydians and Medes in Asia Minor -584, May 28; also of the landing-place of Agathocles on the African shore; lastly, the correction of the longitude of Nimrûd, as given in Capt. F. Jones' map.

Appendix F.

Suppose that Hansen's value is practically required, and that the difference between the theoretical and the practical value is caused entirely by the retardation of the Earth's rotation, Hansen, in the remarks already quoted, tacitly assumes, that, because the time of rotation becomes variable, therefore the length of the mean day, which is the unit of time of the tables, becomes also variable. Now, this is not at all such a matter of course. If astronomers agree to retain as the unit of time a mean day of *uniform* length, the whole effect of the retardation of rotation is thrown upon the tables for converting sidereal time into mean time, and the tables of the Moon and planets remain perfectly unaffected by the variability of the rotation, and the Lunar Tables must therefore contain the strictly theoretical value of the acceleration:—this course of taking account of the retardation seems to be more simple than the other.

Before retiring from the high and honourable position to which, for two successive years, you have been pleased to call me, and resigning into the able hands of my successor the responsible duties of the Presidency, I desire emphatically to assure you how fully I have always appreciated the generous support on all occasions accorded me by my Colleagues in the

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Council, and by yourselves in this Room. I have never occupied this chair without feeling that I was in the presence of considerate friends cordially sympathising and co-operating with me in my continual, however imperfect, endeavours to make our meetings both instructive and agreeable, and ever ready to condone, with their indulgent kindness, my inevitable shortcomings. To each and to all I tender my grateful thanks.

During my tenure of office one or two innovations have been introduced, to which I now refer simply for the purpose of stating that they were purely personal, and in no way binding on any future occupant of the President's chair. One of these is, the report of the *vivâ voce* discussions at our meetings; the other is, the annual *Réunion* of our own Members and the members of other scientific bodies, as well as of other gentlemen of eminent position in political or social life. It was a matter of great gratification to me to meet so many distinguished friends and cultivators of science; and I believe that such assemblages are productive of good; but at the same time I wish it to be understood that I had no desire to establish a precedent.

And now, let me add that my two years of office have been to me a source of the highest and purest pleasure.

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The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

Rev. CHARLES PRITCHARD, M.A. F.R.S.

Vice-Presidents:

Rev. Professor CHALLIS, M.A. F.R.S.
WARREN DE LA RUE, Esq. F.R.S.
JOHN RUSSELL HIND, Esq., F.R.S., Superintendent of the *Nautical Almanac*.
Rev. ROBERT MAIN, M.A. F.R.S. Radcliffe Observer.

Treasurer:

SAMUEL CHARLES WHITBREAD, Esq., F.R.S.

Secretaries:

RICHARD HODGSON, Esq.
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G. B. AIRY, Esq. M.A. F.R.S. Astronomer Royal.
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WILLIAM HUGGINS, Esq. F.R.S.
JOHN LEE, Esq. LL.D. F.R.S.
Captain WILLIAM NOBLE.
J. NORMAN LOCKYER, Esq.
Major-Gen. SHORTREDE.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *March* 9, 1866. No. 5.

Rev. CHARLES PRITCHARD, President, in the Chair.

John Matheson, Esq., Glasgow;
Jabez Moden, Esq., Wellington Street, Gloucester;
Rev. D. W. Durnell, Welton, Northamptonshire; and
Dr. Dodgson, Cockermouth,

were balloted for and duly elected Fellows of the Society.

Investigations on Airy's Double-Image Micrometer.
By Prof. F. Kaiser, Director of the Observatory at Leyden.

The director of the Radcliffe Observatory, Oxford—the Rev. R. Main—has desired that I should present to the Royal Astronomical Society a brief account of my investigations of Airy's Double-Image Micrometer, and I feel honoured in satisfying that desire. These investigations form however only a small part of my investigations on micrometer measurements in general, which I can still by no means consider as closed, and the complete description of which would require a whole volume. Therefore I must limit myself to mentioning a few results, that may be of some value for those who possess and use Airy's Double-Image Micrometer, and I hope at least to

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give hereby a proof of the high value which I attach to this important English invention.

Already in the year 1855 Mr. W. Simms made, at my request, a double-image micrometer of Airy's invention, with a position-circle, for the Observatory at Leyden. This instrument was destined to be added to my 6-inch refractor there, and to serve for the measurement of planets, for which the wire-micrometer appeared less appropriate. Though Mr. Simms has delivered the instrument for the moderate price of 16*l.* 16*s.* it is very beautifully executed, and the modification of its construction that appears to me desirable would not be possible without considerably increasing its price.

The Astronomer Royal himself has given, in the *Greenwich Observations*, in the *Memoirs of the Royal Astronomical Society*, vol. xv., and in the *Monthly Notices of the Royal Astronomical Society*, vol. x., a complete theory and description of his double-image micrometer, therefore I may consider the instrument as sufficiently known. From that theory and description it appears that, with the double-image micrometer, errors are to be feared arising from the following sources: —(1.) Periodical errors of the micrometer screw; (2.) Variability in the mutual distance of the threads of that screw; (3.) Distortion of the images. The periodical errors of the screw may be very different at small differences in the readings of the micrometer-head, and require therefore a particular inquiry. The errors arising from the sources (2) and (3) do not compensate themselves in brief periods, and their common amount may be determined by the same inquiry.

In the micrometer of Simms one half-lens is quite fixed, while only the other can be moved by the micrometer-screw. This construction makes it impossible to eliminate the periodical errors of the screw at the measurements themselves. If the fixed half-lens could only be moved so much as the amount of one revolution of the micrometer-screw, we could hereby, according to Bessel's theory, eliminate at each measurement the periodical errors of the screw, and this would be a great gain. If the fixed half-lens could be moved as much as the moveable one, the measurements could be extended to angles twice as large as can now be measured with the micrometer. Then it would however be necessary that the two half-lenses should be shown together, with relation to the fixed lenses. By this modification the price of the instrument would certainly be considerably raised, but it would be worth while.

I should require too great a space to describe here in what manner I have determined the periodical errors of the screw of Airy's micrometer, while the instrument is not adapted to this inquiry. Therefore I will only mention the results obtained, from which it appears that the periodical errors of the screw are very considerable, and very different in its different parts. The scale by which the revolutions of the

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screw are counted is extended from 0 to 50. In the year 1863 I demonstrated, by many hundreds of measurements, the periodical errors of the screw, from 5 to 5 revolutions, between the readings of the scale 10 and 40. If the periodical error be expressed, as usual, by the periodical function:

$$\varphi(u) = a \cos u + b \sin u + c \cos 2u + d \sin 2u +$$

in which u signifies the reading of the micrometer-head, transformed into degrees, according to my inquiry, we have for the periodical errors of the screw in its different parts, the following values:—

Scale 10 $\varphi(u) =$	$-1{\cdot}075 \cos u$	$-0{\cdot}578 \sin u$	$-0{\cdot}149 \cos 2u$	$-0{\cdot}022 \sin 2u$
15	$-0{\cdot}806$	$-0{\cdot}934$	$-0{\cdot}183$	$-0{\cdot}025$
20	$-0{\cdot}720$	$-0{\cdot}975$	$-0{\cdot}216$	$+0{\cdot}046$
25	$-0{\cdot}721$	$-1{\cdot}283$	$-0{\cdot}146$	$+0{\cdot}051$
30	$-0{\cdot}655$	$-1{\cdot}338$	$-0{\cdot}142$	$+0{\cdot}068$
35	$-0{\cdot}835$	$-1{\cdot}527$	$-0{\cdot}140$	$+0{\cdot}135$
40	$-0{\cdot}729$	$-1{\cdot}711$	$-0{\cdot}310$	$+0{\cdot}195$

It appears, therefore, that the errors of the screw are very variable, and, in themselves, five times larger than those of Bessel's heliometer.* The value of one revolution is, however, for the heliometer 52″ and for Airy's micrometer, according to the different powers, from 5″ to 11″, so that the influence of these errors is from 5 to 10 times smaller than with the heliometer. The errors are however far too great to be neglected.

The common amount of the errors (2) and (3) is obtained by determining the value of the revolution of the screw for different values of the measured angle, or, which is the same thing, for different distances of the half-lenses. This amount will be imperceptible if, for all the distances of the half-lenses, there is found the same value of the revolutions of the screw, and then the movement of the half-lens will be sensibly proportional to the size of the objects that are to be measured. It is however extremely difficult to determine, with the required accuracy, the value of the revolutions of the screw. Even though Bessel had not positively declared it,† we could easily be convinced, by our own experience, that no observation of transits, or angle-measurement with a circular measuring instrument, can

* Bessel, *Astronomische Untersuchungen*, vol. i. p. 83.

† Bessel, *Astronomische Nachrichten*, No. 403, and *Astron. Untersuchungen*, vol. i. pp. 51 and 132.

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give a micrometrical accuracy. Bessel preferred a heliometer, even for measuring small angles, because with that instrument it is possible to embrace also angles of more than one degree. If the value of the revolutions of the screw were determined by the ordinary means for a space of more than one degree, the inevitable error committed in this would be considerably diminished in angles of a few minutes, which are peculiarly to be measured by the instrument. With Airy's micrometer it is hardly possible to exceed the small angles that are to be measured therewith, and the errors committed in determining the revolutions of the screw are therefore not diminished in the results of the measurements which are to be performed with the instrument. I had requested therefore that Airy's micrometer, that is, a terrestrial eye-piece, should be constructed in such a manner that it might be directed on the wires of the wire-micrometer. The distance of the wires can be determined with the wire-micrometer free from all errors of the instrument. If that distance be measured again with Airy's micrometer, we might, with a micrometrical accuracy, derive therefrom the value of the revolutions of the screw, and Airy's micrometer would give the same accuracy as the wire-micrometer, in those cases in which the latter instrument, on account of the diffraction of light, does not allow a sharp definition. Unfortunately Airy's micrometer could only be adapted to the wire-micrometer with its two lowest powers, which are too small to allow any sharp measurements.

Airy's micrometer has four first lenses, with focal distances of $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, and 1 inch, which give, with the 6-inch refractor, magnifying powers of 326, 220, 143, and 109 times. Already in the year 1856 the value of the revolutions of the screw was determined for each of those powers, and for very different values of the measured angle. For doing this, with the two highest powers, the wire-micrometer was screwed on a refractor of Steinheil having an aperture of 4 inches and a focal distance of 9 feet, while Airy's micrometer was adapted to the 6-inch refractor. The object-glasses of both the refractors were directed on each other, and with Airy's micrometer the image of the wires of the wire-micrometer formed by both the object-glasses was measured. For the two lowest powers the micrometer of Airy was immediately adapted to the wire-micrometer. Thus the value of the revolutions of the screw was determined, with great care, for the different powers and for different measured angles, and I gave an extensive report of these investigations, with a number of remarks on Airy's micrometer, in an ample treatise that was, in the year 1857, published by the Royal Academy of Sciences at Amsterdam.*

After the foundation of the new Observatory at Leyden, in

* The title of that treatise, *Eerste onderzoekingen met den Mikrometer van Airy, volbragt door F. Kaiser.* Amsterdam: C. G. van der Post. 1857.

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the year 1861, the 6-inch refractor was exclusively destined to extra-meridional observations of planets and comets, and the new 7-inch refractor of Merz was destined to micrometer measurements. Airy's micrometer was adapted to the 7-inch refractor, and, in the year 1863, I undertook a new investigation of Airy's micrometer, in quite a different manner from the former, in order not to be obliged to dismount the instrument for it. To the edifice of the University, at a distance of 240 metres from the refractor, I fixed a heavy black shelf, to which were attached some silvered copper disks. Two of these disks were very small, and were placed at a mutual distance of about 0·50 metres. The remaining disks had diameters of about 0·01, 0·02, 0·03, 0·04, 0·05, and 0·06 metres. The linear dimensions of these diameters, and of the distances of the small disks, were accurately measured, and this made known the ratio between these quantities. The angular value of the distance of the small disks was afterwards determined by the wire-micrometer, and, by the said ratio, from this, by proportion, the angular value of the diameters of the larger disks was derived. The angular distance of the smaller disks, amounting to above 7′, could be determined with a certitude of about 0″·1; and, as the diameters of the disks were from 8 to 50 times smaller, the angular values of these diameters became known with a certitude of 0″·01 or 0″·02. The diameters of the disks were measured again with Airy's micrometer, and so the values of the revolutions of the screw were determined for different distances of the half-lenses. When however I had measured two of these disks, with all the powers of the micrometer eye-piece, and each of them 50 times, my shelf with disks disappeared, without my being able to discover who had taken it away, and my investigation was broken off.

In the past year I had new disks made that were fixed to a piece of iron, and were attached to a part of the edifice of the University, where they were more safe than before. The measurements were repeated in the former manner, and extended with Airy's micrometer, on five disks, with diameters of 8″ to 45″. The result for each disk and each power rested on no less than a hundred pointings and readings. The silvered disks however soon gathered moisture, and they were soon so little brighter than the blackened iron that the measurements were extremely difficult.

Lately I effected quite a new investigation with respect to Airy's micrometer, in which I superseded the former disks by artificial double-stars. To the above-mentioned iron were screwed copper-plates with little apertures that appeared, by the refractor, under an angle of 0″·8, and at such distances as that they represented double-stars at distances of 7″·5 to 74″·6. Behind the iron was placed a mirror that reflects the daylight through the apertures towards the refractor, and by which these apertures show themselves very clearly with a clouded

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sky. After these artificial double-stars were measured lineally with great care, I measured each of these objects 50 times with the second power of Airy's micrometer.

All the above-mentioned inquiries have, without an exception, led to the result that the value of the revolution of the screw of Airy's micrometer increases with the measured angle, and that therefore the movement of the half-lens is not proportional to the distances of the images, and that a considerable error may be committed by assuming a constant value for the revolution of the screw. I cannot communicate my thousands of measurements for the investigation concerning Airy's micrometer with any completeness without requiring a space that I cannot dispose of here, but, as a proof, I communicate the measurements performed with Airy's micrometer, in the last year, with the second power, on eight artificial double-stars. Each of these objects which I shall name *a*, *b*, *c*, *d*, *e*, *f*, *g*, and *h*, was each day measured five times, and these measurements were repeated on ten different days, so that each result rests on a hundred readings. The results obtained each day, and expressed in revolutions of the screw, are the following:—

	a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
	0·9862	2·0133	3·0023	3·9995	5·2918	6·2845	7·2445	9·7666
	0·9924	2·0084	2·9943	3·9805	5·3083	6·2898	7·2382	9·7657
	1·0019	2·0009	3·0067	3·9909	5·3026	6·2957	7·2611	9·7792
	0·9954	2·0055	3·0002	3·9858	5·2856	6·2948	7·2448	9·7658
	0·9891	1·9967	3·0148	4·0083	5·3001	6·2988	7·2673	9·7667
	0·9975	1·9993	3·0072	3·9887	5·3188	6·2889	7·2745	9·7715
	0·9977	2·0065	3·0065	3·9932	5·3011	6·2944	7·2709	9·7810
	1·0041	2·0088	3·0192	4·0098	5·2993	6·3089	7·2810	9·7907
	1·0068	2·0131	3·0113	4·0044	5·3098	6·2974	7·2626	9·7015
	1·0149	2·0281	3·0207	4·0048	5·3248	6·2978	7·2630	9·7873
Mean	0·9986	2·0081	3·0083	3·9966	5·3042	6·2951	7·2608	9·7756

Airy's micrometer gives, with the 7-inch refractor, powers of 413, 278, 178, and 137 times. For the above-mentioned measurements the second power, namely that of 278, was used. If it be remarked that, with that power, the value of a revolution of the screw amounts to about 7″·5, it immediately appears from the above measurements that Airy's micrometer admits of a great accuracy, and therefore deserves a rigorous investigation. The above results require however still a small correction that arises from the manner in which I performed the measurements. If we call the images formed by the one half-lens *m* and *n*, and those formed by the other *m′* and *n′*, *n*

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and m' were brought, as nearly as possible, under each other, and placed so that the line passing through their centres stood perpendicularly to the line that connects m and n' together. This was done by the moveable half-lens on the left-hand; by the moveable half-lens on the right-hand the same thing was done for m and n'. Of course the micrometer-screw was always turned in the same direction. Half the difference of the readings would have been precisely the distance of the centres if the images fell into the same straight line, but now it is a little too small. I chose this manner of measuring because it appeared to me it allowed a very sharp definition, and excluded the possibility of constant error. After applying the necessary small corrections, I found, for the true distances in revolutions of the screw, the following final results:—

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
1·0043	2·0109	3·0101	3·9980	5·3053	9·2959	7·2616	9·7762

With a comparator that I had composed from parts of other instruments, I repeatedly measured the linear distances of the different artificial double stars, and I got for this the following results, expressed in millimeters:—

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
8·625	17·455	26·253	34·915	46·365	55·195	63·820	86·290

The distance of the two most distant small apertures was 480·038 millimeters, and by often repeated measurements with the wire-micrometer in which the periodical errors of the screw were eliminated, it was found that this distance amounted to 20·0213 revolutions of the screw of the wire-micrometer. The value of each revolution of that screw had, by very numerous passages of stars, while the wires were 13′ distant from each other, been determined to be 20″·715, and from this ensued, by proportion, for the distances of the artificial double-stars, in seconds:—

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
7″·452	15″·080	22″·681	30″·165	40″·057	47″·686	55″·138	74″·550

From these numbers we find, with the second power of the micrometer, the following results for the value of each revolution of the screw. The number above each result signifies the distance of the half-lenses, expressed in revolutions of the screw, corresponding to the revolution which has been found:—

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a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
1·00	2·01	3·01	4·00	5·31	6·30	7·26	9·78
7″·420	7″·499	7″·535	7″·545	7″·550	7″·574	7″·593	7″·626

From this it appears therefore, that the value of the revolutions of the screw increases very sensibly with the size of the measured angle, and all the investigations of the same nature, performed with the micrometer, have led to the same result.

Constant errors in the measurements might cause different results to be found for the value of the revolutions of the screw at different angles, even if the micrometer were perfect. Thus, for instance, assuming that *a* and *h* have both been measured 3·01 revolutions, or 0″·23 too large, the same result, namely, 7″·649, would be found, but in the actual measurements it was totally impossible to commit a constant error of that amount. The measurements on the remaining artificial double stars are also contrary to the supposition of a constant error. If, namely, we assume the value of 7″·649 for all the artificial double stars, the errors of the measurements should have the following amounts :—

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*
− 0″·23	− 0″·30	− 0″·34	− 0″·42	− 0″·52	− 0″·47	− 0″·41	− 0″·23

As well the amount as the regular increase and diminution of these differences renders the supposition absurd that the value of the revolutions of the screw was constant. Although I was convinced that the errors of the above results could at the utmost amount to a few hundredth parts of a second, I have measured four of the artificial double stars — namely, *a*, *b*, *c*, and *d* — once more in a quite different manner. If we again name *m* and *n* the two objects of the one image, *m′* and *n′* those of the other; I now brought all the objects into the same straight line and placed, with movable half-lens on the left hand, the object *m′* first on the left and then on the right of *n*, so that there remained each time a black space as a *minimum visibile*. Afterwards the same was done by movable half-lens on the right hand with the objects *m* and *n′*, and the distance of the centres was thus obtained by four measurements instead of by two. I repeated the measurement each day five times, and performed this on eight different days. The results obtained on those days are the following :—

a	*b*	*c*	*d*
1·0060	2·0165	3·0199	4·0042
1·0030	2·0132	3·0147	4·0081
0·9959	2·0181	3·0212	4·0045

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	a	*b*	*c*	*d*
	0·9979	2·0095	3·0175	4·0077
	1·0057	2·0156	3·0179	4·0007
	1·0034	2·0153	3·0156	4·0070
	0·9961	2·0106	3·0183	4·0045
	1·0062	2·0134	3·0153	4·0066
Mean	1·0018	2·0140	3·0175	4·0054

The absolute differences compared with the former results are for—

a	*b*	*c*	*d*
+ 0″·02	− 0″·02	− 0″·05	− 0″·05

Although the differences are greater than I expected, they are small enough to exclude the possibility of errors that amount to 0″·52. I prefer the first method of measuring, as it appears to me to allow the greatest accuracy in pointing.

If the value of a revolution of the screw is assumed to undergo a change proportional to the size of the measured angle; if x is named the value of a revolution of the screw for a distance equal to zero, and y the increment of that value for each revolution of the screw, each of the artificial double-stars gives an equation of the form—

$$m = n x + n^2 y.$$

The solution of the eight equations obtained in this manner, according to the method of least squares, gave—

$$x = 7''\cdot4791$$
$$y = + 0\cdot01511$$

and therefore, if r signifies the value of each revolution of the screw, and R the dimension of the measured angle in revolutions of the screw—

$$r = 7''\cdot4791 + 0''\cdot01511\ \mathrm{R}.$$

If we calculate by this the value of r for the different artificial double-stars, we get for—

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	a	*b*	*c*	*d*
Observed ..	7″·420	7″·499	7″·535	7″·545
Calculated ..	7·494	7·509	7·525	7·539
Difference ..	+0·074	+0·010	−0·010	−0·006
Absolute error	−0·07	−0·03	+0·03	+0·02

	e	*f*	*g*	*h*
Observed ..	7″·550	7″·574	7″·593	7″·626
Calculated ..	7·559	7·574	7·589	7·624
Difference ..	+0·009	0·000	−0·004	−0·002
Absolute error	−0·05	0·00	+0·03	+0·02

The amount of the absolute error at *a* seems to indicate that the value of *r* increases more slowly in proportion as R grows larger. This deserves attention, since the second series of measurements also agrees with it.

In order not to make this paper too extensive, I will only mention, as regards my remaining investigations on Airy's micrometer, the final results of the measurements on five metallic disks performed in the year 1865. By very numerous measurements the diameters of these disks were found thus:—

	1	2	3	4	5
In Millimeters	9·987	20·020	30·047	40·027	45·928
In Seconds	8″·628	17″·296	25″·959	34″·581	43″·135

The diameters of these disks were, with the powers 1, 2, and 3 of Airy's micrometer, measured on each day five times, and these measurements were repeated on ten different days. The final results, expressed in revolutions of the screw, are the following:—

	1	2	3	4	5
Power 1	1·7407	3·4496	5·1267	6·8082	8·4488
Power 2	1·1535	2·3124	3·4531	4·5898	5·7117
Power 3	0·7596	1·4983	2·2341	2·9703	3·6930

From this follows for the value of *r*:—

	1	2	3	4	5
Power 1	4″·957	5″·014	5″·063	5″·079	5″·105
Power 2	7·480	7·480	7·518	7·534	7·552
Power 3	11·359	11·544	11·620	11·642	11·680

Consistently with all the investigations performed, we

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see here again r increasing when the measured angle grows larger.

If we assume for the second power the before-found value of r, $r = 7''{\cdot}4791 + 0''{\cdot}01511$ R, we have for r at the different disks of which the diameters have been measured —

	1	2	3	4	5
Observed	7″·480	7″·480	7″·518	7″·534	7″·552
Calculated	7·496	7·514	7·531	7·548	7·565
Difference	+0·016	+0·034	+0·013	+0·014	+0·013
Absolute Error	−0·02	−0·08	−0·04	−0·06	−0·07

It appears therefore that the disks have been measured rather too large, which cannot be astonishing, since by the interference of light they must show themselves rather larger than they are. The result that Airy and Liouville have obtained by measurement, that the apparent diameters of disks are, contrary to theory, independent of the apertures of the refractors, deserves indeed a further inquiry. The mean of the absolute errors is $-0''{\cdot}056$. If we assume this amount, the remaining errors are, with

1	2	3	4	5
−0″·04	+0″·02	=0″·01	+0″·01	+0″·02

The measurements on the disks were sensibly less sure than those on the artificial double-stars, because the former very soon lost their whiteness. The disk 1 especially was generally very faint. I intend to repeat this inquiry with round apertures through which the daylight shines. It will hardly be necessary to remark that all the measurements recorded here are freed from the periodical errors of the screw.

It is easy, by a repetition of observations, to give to a final result of micrometer-measurement, a probable error of only a few hundredth parts of a second, but that probable error is a false measure for judging of the real error. It is ordinarily seen that final results for the same dimension, obtained by micrometer-measurements, differ some tenth-parts of a second from one another; and if it is considered from what complicated operations the above results have been derived, we have reason to be very well satisfied with their mutual harmony. An object with a diameter of one second presents itself, even with a power of 300, still so small to the eye, and the air is, moreover, continually so impure and unquiet, that we must despair of performing micrometer-measurements with a certainty of a few hundredth parts of a second, and I do not believe that this accuracy has ever been obtained, however small the probable error of the final result may be.

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From the above-mentioned investigations it appears that the Astronomer Royal has, by the invention of his double-image micrometer, rendered an important service to Astronomy. I doubt very much whether, besides the Heliometer, a second double-image micrometer exists by which measurements can be effected so accurately as by Airy's double-image micrometer. That instrument requires however a very rigorous and difficult inquiry in order to give it the accuracy of which it is capable, and it requires also great prudence in the use of it. In my treatise of the year 1857 I have treated circumstantially of the precautions that Airy's micrometer requires, and of its properties.

A micrometer like Airy's can only render its services if it is adapted to a large and precious refractor. The price of the micrometer would, even if it were doubled, still remain very insignificant in comparison with that of the refractor. Therefore I should think it very desirable to give to the micrometer the above-mentioned more complicated construction, even if its price were thereby considerably increased. It would be very important if the periodical errors of the screw could be eliminated by the observations themselves, and, by a mobility of the fixed half-lens, the measurements could be extended to angles twice as large as those which the micrometer is now capable of measuring.

Leyden, Feb. 1866.

Additions to the Investigations on Cometary Systems.

By M. Hoek.

In § 11 of my paper "On the Comets of 1677 and 1683, 1860 III., 1863 I., and 1863 VI." present volume, pp. 1–12, I announced the necessity of a new combination of our whole stock of cometary aphelia with adequate enlargement of the limit of time. I have effected this investigation.

In order to have before the eye the whole of the phenomena furnished by the distribution of cometary aphelia and planes of orbits, I have made a drawing containing 190 orbits of comets discovered since the year 1556.* First, I laid down in it the aphelia whose positions were obtained by calculation after the elements that seemed to be the most reliable. Further I traced through these aphelia the circles which re-

* Periodical comets and elliptical as well as uncertain orbits have been excluded.

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present in a stereographic drawing the intersections of the planes of the orbits with the sphere. But, in order to avoid a confusion which would have been intolerable, I limited these circles to a length of 10° in average on both sides of the aphelion.

It was the intention to find out those points of the sphere in which occurred an intersection of several orbits, the aphelia of which were distributed around these intersectional points. For, having discovered such points, we have reasons to presume that they indicate, each of them, a centre of cometary emanations, such as the point

I. ..... $\lambda = 319°$ $\beta = -78°.5$ Mean Equinox of 1864·0.

the discussion of which was given in my former papers.

I find that it is possible that there are such centres in the following positions:—

II.	$\lambda = 267°.0$	$\beta = -51°.6$	Mean Equinox of 1864·0
III.	$\lambda = 175°.5$	$\beta = -46°.5$	
IV.	$\lambda = 75°.5$	$\beta = -51°.7$	
V.	$\lambda = 274°.6$	$\beta = +38°.7$	
VI.	$\lambda = 92°.9$	$\beta = +0°.6$	

and I proceed to indicate what there is remarkable in each of them.

II. is the intersectional point common to the orbits of the comets of 1739, 1793 II., 1810 and 1863 V. Unfortunately for our purpose, there is uncertainty with respect to one of these comets. It is possible, and even somewhat probable, that the Comets of 1810 and 1863 V. are two apparitions of a same body revolving every 53·3 years.

III. is the centre of a small circle of 1° in diameter, through which pass the orbits of the Comets of 1764, 1774, 1787, and 1840 III.

IV. is the centre of a small circle of 1° in diameter, through which pass the orbits of six comets, those of 1596, 1781 I., 1790 III., 1825 I., 1843 II., and 1863 III. At a small distance from this circle pass the orbits of the Comets of 1785 II., 1818 II., and 1845 III. But what gives to its centre a peculiar interest is that it is only 1°·5 distant on the sphere from the point

$\lambda = 73°.2$ $\beta = -51°.6$ Mean Equinox of 1864·0

in which occurs the intersection of the orbits of the Comets 1857 III. and 1857 V. As for me, I do not doubt that these two comets are members of a former system, when I

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consider how small the interval of time was between their apparitions, and how uncommonly great the resemblance is in all the elements of their orbits.

It is true that among the eleven orbits, which may be connected with the point IV., there are some whose aphelia are distant from it by more than 10°, the average distance I had adopted. But we must remember, first, that there is something arbitrary in this limit; secondly, that we ought not to consider as exactly known the aphelia such as they are given by the computation of the orbits. Let us remember that often it is possible to represent the observations of a comet in different manners, by varying the longitude of the perihelion, and according to it the time of its passage. Further, that very often, when the comet is still at a great distance from us, the observations are rendered less certain by the circumstance that no nucleus is visible. Finally, that generally when the comet has approached our earth, and when we perceive its nucleus, its movement must necessarily be modified by the reaction it has to suffer from the particles evaporating under the influence of the Sun.*

V. is the centre of a circle of 3° in diameter traversed by the orbits of the Comets of 1773, 1808 I., 1826 II., and 1350 II. Unfortunately the orbit of the second of these is less certain.

VI. is the centre of a small circle of 52 minutes of angle in diameter, within which are accumulated the aphelia of the Comets of 1689, 1698, 1822 IV., and 1850 I.

The probability, *à priori*, of this phenomena being

$$(\sin^2 . 13')^3 \text{ or } 0,000\,000\,000\,000\,002925,$$

we could have expected its occurrence by chance only once in 341900 000 000 000 cases, and all our comets (190) furnish only 52590000 quaternary combinations, that is a $\frac{1}{6500000}$ part of the required number.

It is very unfortunate, indeed, to have here again a reason to doubt. The orbit of the Comet 1689 is rather uncertain, and the allowable variations of the elements would easily remove the aphelion far from the indicated limit of position.

Let us, however, observe that even when the orbit 1689 is wholly rejected, the point VI. retains a deal of its interest.

* It appears even probable to me that we must admit such influences in the aphelia of the Comets 1618 II., 1723, 1798 II., 1811 II., and 1849 I., all lying around the point

VII. ..... $\lambda = 217°.8$ $\beta = +26°.6$ Mean Equinox 1864.0;

in which four of these orbits meet, and which is only distant from the fifth (the orbit of the Comet of 1723) by 2°.1.

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In order to show this, I have repeated the calculation, admitting that only three aphelia are contained within the circle.

The probability of that phenomenon is $(\sin^2 13')^2$ or 0, 000 000 000 2046, and therefore we may expect to find it once in 4889 000 000 000 ternary combinations. Now, that number of cases would only be furnished by more than 3050 comets, that is to say, sixteen times the number which actually has entered into my calculations.

I believe that it is sufficient to have called the attention of astronomers to these points of the sphere. Generally, there is too much doubt about the elements as well as about the periodicity of ancient comets. I do not wish to give here any decisive conclusions, and I expect the confirmation of my views from the future apparitions of such bodies. But, therefore, let me repeat it, it is necessary that their discovery be as much as possible assured.

A single remark finally. My drawing of aphelia shows a very remarkable phenomenon. Let us draw a circle through the points of the sphere

$$\lambda = 95^\circ \qquad \beta = 0^\circ$$
$$\lambda = 169 \qquad \beta = 32$$
$$\lambda = 243 \qquad \beta = 0$$

I say that the sector contained between this circle and the ecliptic is uncommonly poor in aphelia. Indeed, instead of containing fifteen of them, which would be required by a uniform distribution, it only contains one, that of the Comet of 1585, situated at 3° distance from the ecliptic.

How are we to explain this? If we knew that the solar system was removing from the point situated in the middle of that sector, I should be inclined to attribute the phenomenon to a difficulty Comets might experience in overtaking the Sun. But the direction of the solar motion, such as it was given by Mädler's investigations, does not allow of such an explanation. Therefore we may ask if the phenomenon is a real one, and there is in that direction of the heavens a scarcity of centres of cometary emanations; or rather, if it depends on the circumstances under which comets are ordinarily detected, the sector in question being so near the part of the ecliptic occupied by the Sun from July to December.

Utrecht, December 4, 1865.

M. Hoek has communicated the following corrections to his former papers:—

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In the first paper, vol. xxv. pp. 243–251, § 2, the formula

$$\cos\delta = \frac{1}{1+a}\left(\frac{p}{r}+1\right) \textit{ should be } \cos\delta = \frac{1}{1+a}\left(\frac{p}{r}-1\right)$$

In the second paper, present volume, pp. 1–12, § 1, note to the Comets 1672, 1677, and 1683, 1677 is a stranger, *should be* 1672 is a stranger.

§ 2, belonging to the cometary systems of 1860 and 1863, *should be* cometary system.

§ 6, the equations quoted from the *Theoria Motus Corporum Cœlestium*, *v* should be *r*.

Same §, p. 9, line 21 from the bottom, to be reduced to $\frac{1}{8}$th part, *should be* $\frac{1}{6}$th part.

Same §, p. 9, line 6 from the bottom, repetition of perihelia, *should be* repartition.

Notice of the Great Nebula in Orion. With a Drawing.* By the Rev. T. W. Webb.

A comparison of the various representations of the Great Nebula in *Orion*, which have been given for a period of many years, seems to lead inevitably to the conclusion that our knowledge of its real aspect is still far from complete. While Mr. Huggins' most important discovery of its true constitution renders the idea of change more conceivable, it must be admitted that any alterations of form or brightness which may have been in progress since the employment of the telescope are so masked by discrepancies of delineation as to preclude the possibility of drawing any fully satisfactory conclusion. It would be superfluous to refer to the variations which are so well known to arise from diversity of weather and instruments, and eyes and pencils, and especially from the unwarrantable carelessness of engravers: it only remains to be considered whether these are the sole causes of the striking differences by which we are confronted; or whether, after making due allowance for all these sources of error, there may yet be a residuum of actual physical change. No other means of investigating this point seem so promising as the multiplication of designs by different observers, in different climates, and with different optical means. From such an accumulation of testimony we might hope to deduce a much closer approximation to the truth.

But if I have, from this impression, been led to attempt a very unpretending sketch, and to beg permission to lay it before the Society, I trust it will be understood that I am so sensible of its deficiencies that I should not have ventured to bring it forward in any other form than that of a suggestion,

* The drawing was exhibited at the Meeting.—Ed.

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and with the hope that while there are so many instruments, on all sides, of greater optical power than my own, this reference to so interesting a subject, may induce some of their possessors to outdo, as they easily may, all that I have attempted here.

The sketch, of which a copy is now offered, was commenced in December 1863, and continued in a very desultory manner and with many interruptions till the present season, when, as the design was carried on into fresh regions, the detection of one or two features which do not appear distinctly in previous delineations, determined me to bring it forward at once, though merely as a temporary and provisional effort, to be perhaps corrected some future day by myself, and certainly improved by others. Had the weather not been so very unpropitious, I believe the region N. of the trapezium might have had a chance of being better represented; unfortunately, this is the very part to which I should wish especial attention to be directed; but I have been unwilling to incur further delay, as the present season for examination will so shortly be passing from us.

In this rough attempt to show some part of what may be visible in a certain circumscribed portion of the nebulosity, with an achromatic of $5\frac{1}{2}$ inches aperture, and an eye of average capacity, the object has been to represent the general arrangement of the luminous haze; and the few stars contained in the sketch are inserted merely for the purpose of more convenient identification and reference.

The three or four distinct masses of light S. of the trapezium were drawn at the end of 1863 and during the ensuing spring.

1864, Dec. 27. I suspected the existence of a narrow dark channel connecting the inner end of the *Sinus Gentilii* with a dark opening lying further on in the same direction, which does not appear in the design of Sir John Herschel, but is found in that of Bond.

1866, Jan. 5. Though the constellation had not risen more than half way to the meridian, and definition was fluttering, I perceived with powers of 111 and 212 a dark rift, of irregular breadth, lying on the whole nearly in a straight line a little S. of the stars 87 and 70 (those represented in the sketch). At the E. end it seemed to communicate with the *Sinus Magnus;* at its opposite extremity it opened into a dusky spot or lake, forming its darkest portion. It appeared broader S.W. of the star 87 than S.W. of 70. About half-way between 65 (the P. star of the trapezium) and 87, but a little in front of the joining line, there is a bright knot, at times seeming to inclose a minute star.

1866, Jan. 11. Though near the meridian, I did not see the dark rift so well as on Jan. 5. I could, however, distinctly make out the "lake" at its W. extremity. Its N. edge in

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passing stars 87 and 70 seems a continuation of the *Sinus Magnus*, and the rift extends probably right through the luminous region W. of the lake. I do not think the projecting end of the *Regio Huygeniana* to the S. quite so conspicuous, as compared with the masses lying N.E. and N.W. of it, as when I sketched it in 1863 and 1864.

1866, January 25. The rift could still be detected, especially by averted vision, notwithstanding a moon two days past quadrature. It seems to be feebly traceable beyond the "lake" to the W. into the dark opening which continues the direction of the *Sinus Gentilii* in Bond's drawing, so as to establish an uninterrupted, though in part very faint, demarcation round two sides of the brightest portion of the nebula, reaching from the mouth of the *Sinus Gentilii* to the bottom of the *Sinus Magnus*. With 450 the rift is still pretty distinct; in the triangle formed by the centre of the new lake, and the stars 67 and 70, the F. side is longer than the P., and the N. the shortest of all.

1866, Feb. 3. The sketch was completed for the present.

I have the less hesitation in bringing forward these particulars as I have the pleasure of knowing that my observations on this rift and the "lake" or opening have been in great measure confirmed by Mr. Knott with his beautiful 7½-inch object-glass; and I, therefore, believe there will be little difficulty on the part of any adequately provided observer in verifying them. As some indication of the extent of my instrumental means I may mention that, under suitable circumstances, I have never failed to make out the *Pons Schröteri* and the nebulosity of the included part of the *Sinus Magnus*, since I first caught sight of it a few hours previous to the great earthquake on the night of Oct. 5, 1863. I have also, during the present season, repeatedly distinguished traces of the *Nebula Oblongata*. The *Lacus Lasselii* I find, however, very difficult; and it is certainly much less conspicuous than the dark opening, or "lake," to which I have directed attention, lying N.W. from the trapezium.

Hardwick Parsonage, Feb. 7, 1866.

P. S.—Having had another opportunity, on February 17, when the weather was favourable, of examining the region S. of the trapezium with a power of 450, I beg permission to send a supplementary diagram, in which it will be observed that an additional "cumulus" has been introduced on the line of the "frons," according to Sir John Herschel's nomenclature. The general appearance of this part was as though the "frons" and "rostrum" consisted of a row of contiguous masses, about six in number, of which only the three preceding the bright star 93 were sufficiently insulated to be distinguished sepa-

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rately. The second and third of these, reckoning backwards from the S. point, formed an equilateral triangle with a cumulus in the interior.

The dark rift or channel was not very distinct on that evening; but the opening into which it leads was fairly visible. I doubted of its continuation beyond the opening through the nebulosity to the west.

Path of a Detonating Meteor. By A. S. Herschel, Esq.

Shooting-stars, it is well known, are so abundantly observed on certain nights of the year, that already, at the end of the last century, the term *meteorode* was applied on this account to the night of the 10th of August. It is suspected that large meteors also make their appearance on fixed nights of the year, although not with the same frequency or regularity as the more constant star-showers. Detonating meteors were observed on the 10th of February, 1772, by Brydone; on the 11th of February, 1850, by the present Astronomer Royal; and on the 9th of February, 1865, by a friend of the writer at Bangalore (S. India); all of which probably belonged to the same zone of meteors circulating round the Sun.

On the 21st of November, 1865, at $6^h\,5^m$ G.M.T., a meteor about three times as bright as *Venus* is at its brightest, and having an apparent diameter of 8′ or 10′, was observed by Mr. Warren De La Rue near Cranford. The meteor rose from the eastern horizon, being surrounded at first, like a comet, by a parabola-shaped halo of light, reminding an observer of the zodiacal light, in its form, but rapidly rising and becoming brighter. When at an altitude of about 40° due east the meteor emerged like a fire-ball from a roman candle, of a blue colour, about a quarter of the apparent diameter of the full Moon in width, and drawing after it a trail of reddish-coloured sparks, $2\frac{1}{2}°$ or 3° in length. As it passed overhead, the outpouring and falling behind of the matter forming the train became distinctly visible. At Wimbledon, near London, where the meteor was seen by Mr. F. C. Penrose, it vanished suddenly, or at any rate very rapidly, about 8° N.W. of *α Lyræ*. No streak appears to have been left, or, if conspicuous, it was hidden by a haze which generally overspread the sky. An account of the meteor having been published, and information from other observers regarding its appearance being requested by Mr. De La Rue, observations from places as far distant as Liverpool were received, as well as accounts from places more near to the mouth of the Thames, where the meteor at first made its appearance in the zenith. From a

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comparison of these accounts it is possible to determine, at least approximately, within small limits of error, the real altitude, position, and velocity of the meteor.

The height of the meteor, at the moment of its first appearance, is determined from six independent descriptions, each of which, separately compared with Mr. De La Rue's original observation of the meteor at Cranford, furnishes an estimate of the real height of the meteor at its first appearance. The mean of all the separate heights is taken as the real height; and the average error from the mean of all the separate heights is seven British statute miles.

The height at disappearance is determined, in an exactly similar manner, from the same six accounts compared with the observation of the apparent place of disappearance of the meteor, as observed at Wimbledon by Mr. F. C. Penrose. The accuracy of the determination is about the same as in the former case, or about seven miles, either above or below the mean. When the nature of the phenomenon is considered, and particularly the difficulty of fixing the exact position of the apparent path of the meteor, as seen by the several observers, it is not by any means surprising that such differences should exist in the separate observations, but rather surprising that such good observations could be made.

The meteor of the 21st of November, 1865, traversed the entire length of the valley of the Thames,—a distance of about seventy-five miles,— from forty-one miles above the Nore to twenty-seven miles above the Earth's surface in the neighbourhood of Henley-on-Thames. On the average of four separate accounts, estimated by different observers between four and ten seconds, the time taken by the meteor to travel the entire distance, about seventy-five miles, was six seconds and a half. On this estimate the velocity of the meteor, relatively to the earth's surface, was about eleven miles per second. The direction of the actual position of the meteor's flight was from a point in the neighbourhood of the constellation *Taurus*, between *Taurus* and the head of *Cetus*. The meteor of the 19th of November, 1861, about to be mentioned, was observed at Woodford as at first stationary for two seconds at a point in *Cetus*.

The distance of the meteor, at the moment of its disappearance, from Wimbledon, collectively determined from these accounts, is about thirty-six miles. At Wimbledon Mr. F. C. Penrose heard a loud report, like that of a cannon fired off at the distance of some miles, distinct enough to be heard very plainly by one other person at Wimbledon, about two minutes and twenty seconds after the meteor had disappeared. Sound, with its ordinary velocity of 1090 feet per second in common air, would take two minutes and fifty-four seconds to travel the entire distance of thirty-six miles from the point of the disappearance of the meteor to Wimbledon. Considering, as before,

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the difficulty of fixing the exact position of the apparent path of the meteor, and hence the approximate nature of the real path concluded from the independent statements of the observers, the agreement of the calculated time with the time observed by Mr. Penrose, between the disappearance of the meteor and the occurrence of the sound, must be regarded as a near coincidence. There can be little doubt from this circumstance,— from the nature of the sound, the great apparent brightness of the fire-ball, and from its near approach to the earth,— that this meteor was really a detonating fire-ball.

Detonating meteors are described in the British Association Reports as having taken place in England, during the last five years, very nearly on the same date of the year as the meteor of the 21st of November, 1865. The first of these meteors exploded over Ipswich and Norwich, with three distinct reports, like heavy ordnance or distant thunder, on the evening of the 19th of November, 1861. The sound of the explosion of the second was heard at Hallaton, in Rutlandshire, on the 20th of November, 1864, like distant artillery, lasting several seconds. Finally, a detonating meteor of unusual size, described by Kepler in his *Ephemerides* (cited by Halley in the *Philosophical Transactions* for the year 1719, p. 978), which passed over Germany with a report like thunder, heard in Austria, on the 17th of November, 1623 (N.S.), may be thought, with great probability, to belong to the same zone of meteors encompassing the Sun. The sidereal year exceeding the length of the tropical year by almost exactly one day in seventy years, the position of the Earth in its orbit at the time of the occurrence of Kepler's meteor (nearly two centuries and a half ago) would coincide with the Earth's place in its orbit, at the present time, on the 21st of November; exactly the date when Mr. Warren De La Rue's meteor was observed.

The epochs of the 9th–11th of February, and the 19th–21st of November are, therefore, dates deserving special attention, partly with a view of determining for the future the directions of the detonating meteors, and partly as showing, by their frequent return within very narrow limits of time about those dates, that aërolitic meteors, like the acknowledged star-showers of August and November, revolved in fixed orbits round the Sun.

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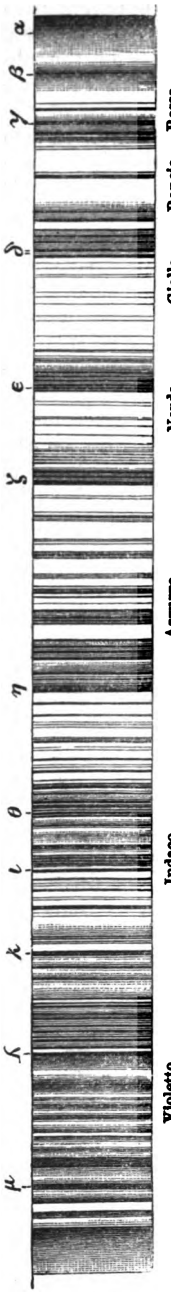

Spectrum of α Orionis. By the Rev. Father Secchi.

(*Extract from a Letter addressed to the Secretary.*)

I take the liberty of sending a sketch of a drawing of the spectrum of *α Orionis*. It has been made with an apparatus of M. Merz, of Munich, combined with a prism of Amici, made by Hoffman. The details are strong and wonderful, although the prism is very small. I hope to be able to make new observations with a larger prism and yet greater light.

As a point of reference, I mention that the line δ is that of sodium, and ν that of magnesium.

There is some considerable difference in the position of some nebulous bands, as compared with the figure of Mr. Huggins'. I am sure there is no fault in the measures. Is there any real change?

The spectrum of *Sirius* is seen with this instrument most admirably. It appears all striated by narrow equal bands, like the spectrum of *Sulphur* in the plate of M. Plücker, given in the *Philosophical Transactions* for the year 1865.

I suspected some illusion in these equal bands, and I looked to *Rigel*, but I found that there also are the bands, but much narrower, almost as the spectrum of nitrogen in the same plate; $4\frac{1}{2}$ bands of *Sirius* are equivalent to $6\frac{1}{2}$ of *Rigel*.

I send you also a photograph of a solar spot observed on the 16th January last. It will show how the details and veils are seen with the polarising eye-pieces. The red colour of some veils vanishes with common eye-pieces, even if diagonal.

Rome, Feb. 10, 1866.

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Note on the Spectrum of the Variable Star α Orionis, with some Remarks on the Letter of the Rev. Father Secchi. By William Huggins, F.R.S., and W. A. Miller, M.D., LL.D.

P. Secchi, in consequence, as he states, of "some considerable difference in the position of some nebulous bands" in his diagram from the figure which accompanies our paper, "On the Spectra of some of the Fixed Stars,"* asks, "Is there any real change?"

A real change in the spectrum of a star, since it implies either a change in the chemical constitution of the star, or important physical changes which effect an alteration in the investing atmosphere of vapours, to the absorptive action of which the lines in the spectrum are due, should not be assumed, except as the result of observations made with the extreme care that so delicate an investigation requires. We do not think that it is possible to determine a question of so much importance to cosmical science from a comparison of P. Secchi's diagram with our own, for the reasons which follow.

P. Secchi gives no information as to how many of the lines in his diagram have been laid down from actual measurement. Into our diagram those lines only were admitted of which the measures are contained in a table which accompanies the diagram. Our figure, therefore, is not intended as a *full* representation of the spectrum *as it appeared* in the instrument,—for a number of lines, much greater than the eighty measured lines given by us, might easily have been inserted, if we had not thought that the trustworthiness of the diagram would have been impaired by the addition of lines of which the positions were only estimated. Our measures were afterwards checked and verified by the simultaneous comparison, in the instrument, of the star spectrum with the bright lines of sixteen elementary substances. The positions of several of the brightest lines of each of these elements are given in our diagram relatively to the measured lines of the star.

Again, since the relative proportions of the different parts of the spectrum are affected by the quality of the glass of which the prisms are made, and also by the positions of the prisms in relation to the light incident upon them, the diagram of *α Orionis* should be accompanied by measures with the same instrument of some well-known lines of reference, as the principal lines of Fraunhofer. P. Secchi says that "the line δ is that of sodium, and η that of magnesium," but with these positions the colours placed beneath the spectrum are not in agreement. In that case, the part of the spectrum under which "azzuro" is written, would be about E of the solar

* *Phil. Trans.* 1864, p. 413.

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spectrum, and on the same scale, the part of the spectrum above "violetto," would be a little more refrangible than Fraunhofer's F.

It may be well to mention here that for the measurement of stellar spectra a very narrow slit must be employed. For since it is not possible by the clock-motion of an equatoreal instrument to maintain the image of a star in an invariable position within the jaws of a rather wide slit, the measures will not be satisfactory, unless the width of the slit is less than that of the image of the star made linear by the cylindrical lens, and the passage of the stellar image before the slit is seen to make no sensible alteration in the position of the lines of the spectrum.

The spectroscope, with which our measures were made, has been preserved with its adjustments unaltered. With this instrument, on the 6th and 12th of March, we measured the more refrangible terminations of the shaded groups of lines which, in our diagram and table of measures, are denoted by the numbers 840, 920, 1169·5, 1300·5, 1420. The identity of our recent measures with those which we made during the first three months of 1864, proves that no change has taken place in the positions of these groups during the last two years. The "shading as of fine lines" is still seen, as in our diagram, on *both sides* of the lines coincident with those of sodium.

An important change has, however, been recently observed by us in the spectrum of this star, though not a change in the *position* of any of the groups, such as has been suggested by P. Secchi.

α Orionis is a variable star of great irregularity, both of period and of extent of change of brightness.* Now we have recently found that the group of lines and "shading as if of fine lines" terminated at its more refrangible end by the strong line, No. 1069·5 in our diagram, is not at present visible in the spectrum of the star. The absence of this group is of great interest in connexion with the variability of the star's light, especially as the time of the disappearance of this group coincides with the epoch of the maximum brilliancy of the star.

Since we might be biassed in our estimation of the present degree of brilliancy of the star, by our knowledge of this peculiar change in its spectrum, one of us wrote to Mr. Baxen-

* Sir John Herschel writes:—"The variations of *α Orionis* which were most striking and unequivocal, in the years 1836–40, within the years since elapsed, became much less conspicuous. In Jan. 1849, they had recommenced; and on Dec. 5, 1852, Mr. Fletcher observed *α Orionis* brighter than *Capella*, and actually the largest star in the northern hemisphere; *Outlines of Astronomy*, p. 602. Until the present time, the changes of this star appear to have been again inconsiderable. See Schmidt, *Ast. Nach.* No. 1570."

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dell, whose successful prosecution of this branch of astronomy is so well known, requesting him to state in what phase of its period of variation *α Orionis* is at the present time. His reply is as follows:—"March 9, 1866. The variable *α Orionis* is irregular both in the extent of its variation, and in the duration of its period. It has been increasing in brightness since the end of December, and is now at, or very near, a maximum. I have often thought that its light was at times variable in colour as well as intensity, being sometimes very perceptibly more ruddy than at others. It is, however, a difficult star to observe, owing to its brilliancy and its ruddy colour, which is differently acted upon by haze or moonlight to that of a white star."

The variation in colour, so well described by Mr. Baxendell, corresponds exactly to the change in the colour of the star, which would be produced by the absence or presence of the group of lines of which we are speaking, since the position of this group in the spectrum is about the boundary of the "orange" towards the "yellow."

We have been for some time occupied with observations of other variable stars, which we believe may give to us new information of the probable mode in which, in some stars at least, the periodic alteration of light of these remarkable objects is brought about. We should, therefore, have much preferred reserving, for the present, our observations of the remarkable fading out of this group of lines in the spectrum of *α Orionis*. We may mention here that the fine lines in the spectra of *Sirius* and *Rigel*, referred to by P. Secchi, were described by us in our paper read before the Royal Society in May, 1864, in the following words :—

"*Sirius*. The spectrum of this brilliant star is very intense, but owing to its low altitude, even when most favourably situated, the observation of the finer lines is rendered very difficult by the motion of the earth's atmosphere. . . . . Three, if not four, elementary bodies have been found to furnish spectra in which lines coincide with those of *Sirius*, viz. sodium, magnesium, hydrogen, and probably iron. . . . . The whole spectrum of *Sirius* is crossed by a very large number of faint and fine lines. . . . . It is worthy of notice that in the case of *Sirius*, and a large number of the white stars, at the same time that the hydrogen lines are abnormally strong as compared with the solar spectrum, all the metallic lines are remarkably faint."

"*Rigel*. A spectrum full of fine lines."

The appearance of these fine lines is not such as to suggest to us any resemblance to those spectra of sulphur and nitrogen which are referred to by P. Secchi.

* *Phil. Trans.* 1864, pp. 427, 428, 429.

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Occultation of 130 *Tauri.* By C. G. Talmage, Esq.

1866, February 23. Occultation of 130 *Tauri*.

G. M. T. of Disappearance,

$= 5^h\ 30^m\ 51^s.73$.

Star rather faint.

G. M. T. of Reappearance,

$= 6^h\ 23^m\ 54^s.26$.

Came out exceedingly bright and sharp for a 6th mag. star; time exact.

Note on the Companion to Antares. By D. A. Freeman, Esq.

I beg permission to say that, on turning my Cooke's $4\frac{1}{2}$-in. refractor on *Antares* an hour before sunrise, on the 22d inst., the atmosphere being in an exceptionally fine condition, the small star was very clearly visible, and, at times, quite free from the light round the large star. The colour of the small star appeared to be green; and, with a negative eye-piece magnifying 180, I kept it steadily in view till the Sun had appeared upon the horizon.

This star being clearly visible with a small object-glass of $4\frac{1}{2}$ inches makes it a matter of surprise that it should have been discovered but recently, only though, from its low declination, it is probably more difficult to see it well in England than in this latitude.

Mentone, Alpes Maritimes, Jan. 24, 1866.

Ephemeris of Iris for the Opposition of 1866. By F. Brunnow.

The following Ephemeris for the opposition of *Iris* has been calculated by me from my Tables. In constructing these, the perturbations of the first order produced by *Jupiter*, *Saturn*, and *Mars*, as well as the terms depending on the square of the mass of *Jupiter*, have been taken into account. As the elliptical elements of the orbit are pretty accurately known, I expect that the error of the Ephemeris will be small, and that the tables will give the planet's place with sufficient accuracy for a long time to come. They represent six normal places distributed over a space of eighteen years very satisfactorily, the errors being as follows: —

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		Δ cos α.	Δ δ.
		s	″
1847	Aug. 20·0	+0·27	+1·98
1851	Oct. 3·5	−0·25	−1·71
1855	Dec. 14·5	+0·05	+0·51
1860	Feb. 8·5	+0·06	+0·85
1862	Sept. 11·5	+0·12	+1·33
1865	June 10·5	−0·25	+0·71

Ephemeris for Berlin Mean Midnight.

		Hourly Motion.		Hourly Motion.	Log Distance.
	h m s		° ′ ″		
Nov. 18	4 59 38·24	−2·052	+25 25 59·5	−13·82	9·95950
19	58 48·18	2·116	25 20 24·0	14·12	9·95847
20	57 56·66	2·175	25 14 41·5	14·41	9·95755
21	57 3·77	2·230	25 8 52·3	14·69	9·95674
22	56 9·62	2·280	25 2 56·5	14·96	9·95604
23	55 15·30	2·327	24 56 54·4	15·22	9·95544
24	54 17·92	2·369	24 50 46·2	15·46	9·95491
25	53 20·57	2·407	24 44 32·2	15·70	9·95457
26	52 22·36	2·441	24 38 12·8	15·92	9·95430
27	51 23·40	2·469	24 31 48·2	16·12	9·95415
28	50 23·84	2·491	24 25 18·9	16·31	9·95410
29	49 23·82	2·508	24 18 45·1	16·49	9·95416
30	48 23·44	2·521	24 12 7·4	16·64	9·95434
Dec. 1	47 22·82	2·527	24 5 26·2	16·78	9·95463
2	46 22·11	2·529	23 58 41·8	16·91	9·95504
3	45 21·44	2·525	23 51 54·7	17·01	9·95556
4	44 20·93	2·515	23 45 5·4	17·09	9·95620
5	43 20·70	2·500	23 38 14·3	17·15	9·95695
6	42 20·90	2·480	23 31 22·1	17·19	9·95782
7	41 21·64	2·455	23 24 29·3	17·20	9·95881
8	40 23·07	2·424	23 17 36·3	17·20	9·95990
9	39 25·29	2·388	23 10 43·8	17·17	9·96110
10	38 28·42	2·348	23 3 52·3	17·11	9·96241
11	37 32·58	2·303	22 57 2·3	17·04	9·96384
12	36 37·88	2·253	22 50 14·3	16·95	9·96537
13	35 44·41	2·200	22 43 28·7	16·84	9·96701
14	34 52·29	2·142	22 36 46·2	16·70	9·96875
15	34 1·61	2·080	22 30 7·2	16·54	9·97059
16	33 12·45	2·015	22 23 32·2	16·36	9·97253
17	32 24·88	1·947	22 17 1·7	16·17	9·97456
18	31 38·97	1·876	22 10 36·2	15·95	9·97669
19	30 54·81	1·802	22 4 16·1	15·71	9·97890
20	4 30 12·48	−1·725	+21 58 2·0	−15·45	9·98121

Dunsink, Observatory of Trinity College, Dublin.

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ERRATA.

Page 144, line 5 from bottom, *for* 190 H. II., *read* 199 H. II.
— — — 9 — *for* 51 H. I., *read* 52 H. I.
— — — 15 — *for* 38 H. IV., *read* 38 H. VI.
— — — 8 — *omit* 4627 192 H. I.
— 150, — 12, *for* Arlem, *read* Anclam, Pomerania.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *April* 13, 1866. No. 6.

Rev. CHARLES PRITCHARD, President, in the Chair.

Edward John Routh, Esq., Cambridge;
George Hurst, Esq., Edgbaston;
H. Emerson Westcar, Esq., Royal Horse Guards, Knightsbridge; and
Charles Victor De Santy, Esq., 34 Radnor Street, Chelsea,

were balloted for and duly elected Fellows of the Society.

On the Supposed Possible Effect of Friction in the Tides, in influencing the Apparent Acceleration of the Moon's Mean Motion in Longitude. By G. B. Airy, Astronomer Royal.

Our illustrious Associate, M. Delaunay, has lately made a communication to the Institute of France, in which he explains a portion of the apparent acceleration of the Moon as possibly due to a real retardation of the rotation of the Earth, and conceives that such retardation may possibly arise from friction in the tidal movement of the waters. In suggesting this explanation, he lays down as fundamental theorems (as I understand) the two following:—First, that, if the solid globe of the Earth were covered with water, there would be high water under the Moon (considered as the only tide-producing body); secondly, that the effect of friction would be to make the semi-diurnal tides later than they would be if there were no friction.

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Any treatment of the tides is, necessarily, very imperfectly applicable to the real motion of the waters, under all their complicated circumstances of unsymmetrical boundaries, varying depths, and unknown laws of friction. Still attempts have been made, upon different hypotheses admitting of mathematical treatment, by which the different points of M. Delaunay's theory may be tested. The attempts to which I refer will be found in the following works:—

I. Newton, *Principia*, Lib. I. Prop. 66, Cor. 19.

II. Laplace, *Mécanique Celeste;* cited also and reinvestigated in *Encyclopædia Metropolitana*, Tides and Waves, Art. (109), &c.

III. Airy, *Encyclopædia Metropolitana*, Tides and Waves, Art. (281) and (325).

Newton's fundamental supposition is that of a fluid ring equatoreally surrounding the Earth; Laplace's, that of a fluid completely covering the Earth, on two suppositions of depth, of which one admits of complete solution and the other admits of imperfect solution; Airy's (among various suppositions), that of a fluid equatoreal ring, in which the motion of the waters may be supposed, either to be unaffected by friction, or to be affected by friction proportional to the velocity of motion (a law of friction which will certainly represent with great accuracy the general effects of fluid friction). And the results of the comparison of the theoretical inferences from these principles with M. Delaunay's assumptions are very remarkable. They are all in direct contradiction to these assumptions, and to M. Delaunay's further deductions:—

IV. Newton, Laplace, Airy, agree in this, that there will be low water under the Moon. In a subsequent part of the *Principia*, Newton thinks that the high water would in some measure follow the Moon's place.

V. Airy shows that the effect of friction is to accelerate the time of each individual tide.

VI. It is a result of this friction that the velocity of the Earth's rotation is not affected.

VII. It is a further result of this friction and the consequent disturbance of the form of the waters that the Moon's motion is affected; her orbit is made to become large, and her motion in longitude is retarded.

The interest now attaching to these points is so great that I may perhaps be excused, even at the risk of repeating an investigation already published, in giving the principal steps of my own process. It possesses the advantage, which at the present moment is of the utmost value, and which is not possessed by any of the other investigations, of including the effects of friction. The results which I have marked (VI.) and (VII.) are new.

The subject of investigation is the motion of the waters in an equatoreal canal of uniform depth; the rotation of the

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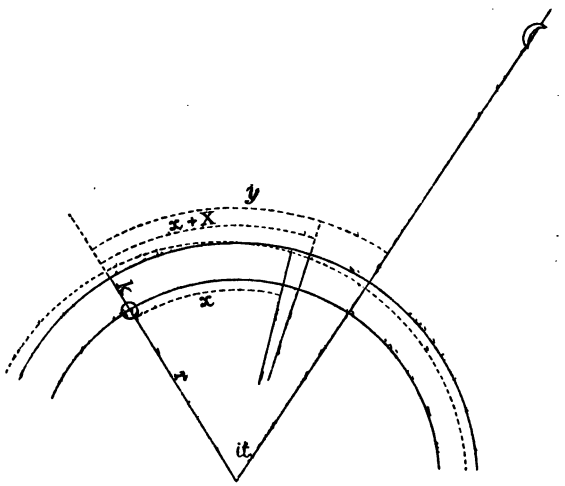
Earth, and the Moon's angular revolution, and consequently the Moon's change of hour-angle, being uniform. The Moon's distance is supposed to be invariable, and her declination either 0 or invariable.

It is easily shown that in this case the motion obtained by supposing the Earth at rest and the Moon revolving with her hour-angle velocity will be the same as that corresponding to the real circumstances (as, indeed, is always assumed).

Adopting some one point of the canal as zero of measure, let x, measured westwardly from that point, be the abscissa for any point of the fluid under consideration; y the similar



abscissa for the point to which the Moon is vertical. If r be the Earth's radius, the angular distance of the Moon westward from the meridian of the point x will be $\frac{y}{r}-\frac{x}{r}$. Then it is known from the ordinary theory of perturbation that the horizontal force produced by the Moon on particles in the place x and its neighbourhood may be represented by $H \sin 2\left(\frac{y}{r}-\frac{x}{r}\right)$ (it is shown in *Tides and Waves*, article 279, that the effect of the vertical force is insignificant). The measure y is proportional to the time, and therefore the expression of this force may be put in the form $+ H \sin (it - mx)$; the positive sign being used because, when $\sin 2\left(\frac{y}{r}-\frac{x}{r}\right)$ is positive, the force tends to move the water in the direction in which x is measured.

A second force is derived from friction. Suppose that the mean abscissa for any particle is x, but that its true disturbed ab-

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scissa at any moment is $x+\mathrm{X}$ (X depending on, or being a function of, x and t). Then the velocity of the particle is $\frac{d\mathrm{X}}{dt}$. And, supposing the friction proportional to the velocity, and the direction always opposed to the direction of motion, it may be represented by $-f\frac{d\mathrm{X}}{dt}$.

A third force depends on the form of the surface of the water. Let the mean depth of the water be k. Then, in the mean state of the water, the volume included between the points whose mean ordinates are x and $x+\delta x$, is $k\,\delta x$. But, in the disturbed state, x is changed to $x+\mathrm{X}$ and $x+\delta x$ is changed to $x+\delta x+\mathrm{X}+\frac{d\mathrm{X}}{dx}\delta x$; the distance between the points is now $\delta x\left(1+\frac{d\mathrm{X}}{dx}\right)$; and, as the volume $k\,\delta x$ is still included between them, the depth of the water now is $\frac{k}{1+\frac{d\mathrm{X}}{dx}}=k\left(1-\frac{d\mathrm{X}}{dx}\right)$ nearly, or its surface is raised by $-k\frac{d\mathrm{X}}{dx}$ nearly. This is the tidal elevation of the point whose mean abscissa is x. The tidal elevation of the point whose mean abscissa is $x+h$ will be $-k\left(\frac{d\mathrm{X}}{dx}+h\frac{d}{dx}\cdot\frac{d\mathrm{X}}{dx}\right)$, or $-k\frac{d\mathrm{X}}{dx}-k\,h\frac{d^2\mathrm{X}}{dx^2}$. The excess of the former is $+k\,h\frac{d^2\mathrm{X}}{dx^2}$. This is the height of a head of water which acts horizontally upon the whole depth k of the water, and of which therefore the entire pressure is $+k^2h\frac{d^2\mathrm{X}}{dx^2}$. The volume of water on which it acts is $k\,h$. Hence, by the usual rule connecting pressure with accelerating force, the accelerating force depending on this cause is $+gk\frac{d^2\mathrm{X}}{dx^2}$.

Collecting these three accelerating forces, and forming the usual equation of motion, and remarking that the abscissa to which the motion really applies is $x+\mathrm{X}$, but that, as x is independent of t, the expression $\frac{d^2(x+\mathrm{X})}{dt^2}$ is really $\frac{d^2\mathrm{X}}{dx^2}$;

$$\frac{d^2\mathrm{X}}{dt^2}=+\mathrm{H}.\sin(it-mx)-f.\frac{d\mathrm{X}}{dt}+gk.\frac{d^2\mathrm{X}}{dx^2}.$$

It is impossible (in our present state of mathematical knowledge) to give a general solution of this equation; and it would be useless if we could give it, because it must include every ripple on the sea. But a solution, of the utmost generality in its reference to the periodic forces which produce the tides, will be obtained by the following assumption of corresponding periodic character:—

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$$\text{Let } X = A \cdot \sin(it - mx) + B \cdot \cos(it - mx);$$

and substitute this for X in the left-hand term and in two of the right-hand terms of the equation. We obtain immediately,

$$0 = \{i^2 A + H + fiB - gkm^2 A\} \cdot \sin(it - mx)$$
$$+ \{i^2 B \quad -fiA - gkm^2 B\} \cdot \cos(it - mx):$$

and, as each of these terms must separately = 0,

$$0 = (i^2 - gkm^2) \cdot A + fi \cdot B + H,$$
$$0 = (i^2 - gkm^2) \cdot B - fi \cdot A;$$

from which,

$$A = -\frac{i^2 - gkm^2}{(i^2 - gkm^2)^2 + f^2 i^2} H; \quad B = -\frac{fi}{(i^2 - gkm^2)^2 + f^2 i^2} H:$$

and

$$X = -\frac{H}{(i^2 - gkm^2)^2 + f^2 i^2} \cdot \{(i^2 - gkm^2) \cdot \sin(it - mx) + fi \cdot \cos(it - mx)\}$$

If the constant angle F be determined by the equation $\tan F = \frac{fi}{i^2 - gkm^2}$; where, with a positive denominator, F will always be positive and less than 90°; then

$$\cos F = \frac{i^2 - gkm^2}{\sqrt{\{(i^2 - gkm^2)^2 + f^2 i^2\}}}, \quad \sin F = \frac{fi}{\sqrt{\{(i^2 - gkm^2)^2 + f^2 i^2\}}}$$

and the expression for X becomes,

$$X = \frac{-H}{\sqrt{\{(i^2 - gkm^2)^2 + f^2 i^2\}}} \cdot \{\sin(it - mx) \cdot \cos F + \cos(it - mx) \cdot \sin F\}$$

$$= \frac{-H}{\sqrt{\{(i^2 - gkm^2)^2 + f^2 i^2\}}} \cdot \sin(it + F - mx).$$

The tidal elevation of the surface of the water, or $-k\frac{dX}{dx}$, is

$$= \frac{-mH \cdot k}{\sqrt{\{(i^2 - gkm^2)^2 + f^2 i^2\}}} \cdot \cos(it + F - mx).$$

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If there were no friction, $f = 0$, $F = 0$, and we should have,

$$\text{X without friction} = \frac{-H}{i^2 - g k m^2}, \sin(i t - m x);$$

$$\text{Tidal Elevation of Surface without friction} = \frac{-m H k}{i^2 - g k m^2}, \cos(i t - m x).$$

In order to arrive at a proper understanding of the import of these expressions, it is necessary first to ascertain whether $g k m^2$ is greater or less than i^2.

Now it will be remarked in a preceding sentence that it has been assumed that y is proportional to the time, say $= n t$, in which assumption it is obvious (on inspection of the first diagram) that n is the number of units of linear measure on the Earth's equator over which the Moon passes in a unit of time, and $i t$ or $\frac{2 y}{r}$ is therefore $\frac{2 n t}{r}$, and i is $\frac{2 n}{r}$; where n exceeds 1000 miles per hour, or 1400 feet per second. The value of m is $\frac{2}{r}$. And $g k$ is the product of 32 by the depth of the sea in feet (always using the foot and the second as the units of measure and time). Thus

$$i^2 - g k m^2 = \frac{1}{r^2}\{\overline{2800}^2 - 128 \times \text{depth of sea in feet}\}.$$

This quantity will always be positive except the depth of the sea exceed 12 miles; far exceeding any supposed real depth of the sea. The denominator $i^2 - g k m^2$ therefore is always to be considered positive.

Now when the Moon is vertical over any point x of the canal, $y - x$ (see the diagram) $= 0$, or $2\left(\frac{y}{r} - \frac{x}{r}\right) = 0$, or $i t - m x = 0$. Therefore, if there be no friction, the tidal elevation of the surface $= -\frac{m H k}{i^2 - g k m^2} \times \cos 0$. This is its maximum negative value. Consequently, *it is low water under the Moon.*

Also, if the term $g k m^2$ in the denominator be neglected, *the tidal elevation (which has k for a factor) is proportional to the depth of the sea.*

Theorems equivalent to these were proved by Laplace.

If there be friction, the tidal elevation of the water is $-D \cdot \cos(i t + F - m x)$, (D being the coefficient above). This quantity has its greatest negative value, or it is low water, when $i t + F - m x = 0$, or when $t = \frac{m x - F}{i}$. If there had been no friction, low water would have occurred when $t = \frac{m x}{i}$. The former value of t is the smaller; and therefore

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the phase of low water (and consequently the other phases of the tide) are accelerated by the friction.

The magnitude of the tide is evidently diminished (but constantly, not varying from day to day) by the friction; its denominator being $\sqrt{\{(i^2 - g\,k\,m^2)^2 + f^2\,i^2\}}$ instead of $i^2 - g\,k\,m^2$.

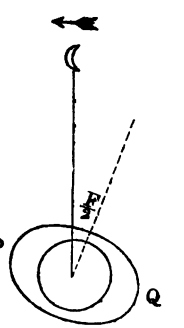
The value of friction upon the water at any place x is $-f \cdot \frac{d\,\mathrm{X}}{d\,t}$; and therefore the reaction of the water's friction upon the solid channel is $+f \cdot \frac{d\,\mathrm{X}}{d\,t}$, or

$$\frac{-\mathrm{H}\,i\,f}{\sqrt{\{(i^2 - g\,k\,m^2)^2 + f^2\,i^2\}}} \cos(i\,t + \mathrm{F} - m\,x).$$

To obtain the effect of this on the entire solid globe, we must integrate $\cos(i\,t + \mathrm{F} - m\,x)$ or $\cos(i\,t + \mathrm{F} - \frac{2}{r}x)$ with respect to x, from $x = 0$ to $x = 2\,r\,\pi$. This definite integral is obviously $= 0$; and therefore *the friction does not tend at any instant either to accelerate or to retard the rotation of the solid globe.*

This result, it will be seen, depends on considerations of the first order only of the disturbing force. The treatment of results to the second order is extremely difficult, and I am unable to say what conclusion would be derived from them. At present it appears desirable to establish, with as much accuracy as the nature of the case permits, the results of the first order.

Yet the friction does produce an effect on the motion of the Moon. We have found that, at the place x, the low water occurs when $t = \frac{m\,x - \mathrm{F}}{i}$, or when y or $\frac{r\,i\,t}{2} = \frac{r}{2}(m\,x - \mathrm{F})$, $= \frac{r}{2}\left(\frac{2\,x}{r} - \mathrm{F}\right) = x - \frac{\mathrm{F}\,r}{2}$. That is, at low water the Moon is east of the meridian, or in the former diagram is to the left of the radius passing through the place of low water, by the angle $\frac{\mathrm{F}}{2}$. The relative position of the Moon and the elliptic water-channel may therefore be represented by this second diagram. The attraction of the protuberance at P tends to draw the Moon transversely to the radius vector, towards the left. The attraction of the protuberance at Q tends to draw the Moon transversely to the right, but by a smaller quantity. On the whole therefore the attraction transversal to the radius vector is to the left, which is the direction of the Moon's real motion. The attraction therefore accelerates the Moon's *linear* motion in its orbit; in consequence of this the Moon's



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orbit is continually growing larger, and therefore the Moon's *angular* motion is diminished; or *the friction of the tides produces a retardation of the Moon's mean longitude.*

It would seem probable that the reaction, on P and Q, of these forces, will in some way produce a retarding effect on the Earth's rotation. There are other instances in the lunar theory in which the Moon's action on the equatoreal protuberance of the Earth is accompanied by action of that protuberance on the Moon, both producing well-recognised effects. But in a case like this before us, where the very existence of the force depends on friction and consequent disturbance of the law of *vis viva*, I do not profess myself able to follow out all the consequences.

Still it is to be remarked that the force, here spoken of, is of a higher order in respect of the fraction $\frac{\text{Earth's radius}}{\text{Moon's distance}}$ than the quantities of which the preceding theory treats; and a more complete investigation will be required before we can pronounce with certainty on the tendencies of the forces to produce secular motions.

It will probably be difficult to say what is the effect of friction in more complicated cases. Conceive, for instance (as a specimen of a large class), a tide-mill for grinding corn. The water, which has been allowed to rise with the rising tide, is not allowed to fall with the falling tide, but after a time is allowed to fall, thereby doing work, and producing heat in the meal formed by grinding the corn. I do not doubt that this heat is the representative of *vis viva*, lost somewhere, but whether it is lost in the rotation of the Earth or in the revolution of the Moon, I am quite unable to say.

The theorem, that when water moves without friction it will be low water under the Moon, is so interesting that I may perhaps be excused for giving a geometrical proof of it (hitherto unpublished). The method of proof will be:—To assume that the ring of water has an elliptic form, the elliptic shape (not the water) travelling round with the same angular velocity as the hour-angle velocity of the Moon, and that the motion of every particle of the water is oscillatory; to examine more precisely the laws of the oscillatory motion of the waters in different parts of the elliptic ring; to investigate the forces which are required for maintenance of these oscillatory motions; and to show that those forces are such as to correspond to low water under the Moon, and to no other relative position of the tide and the Moon.

First, it is to be carefully remarked that the rising of the

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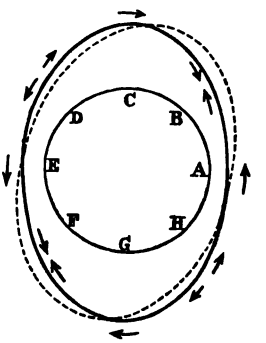
water at any place does not depend on the horizontal movement of the water at that place, but on the relative values of the horizontal movement on the two sides of the place. If the water on both sides of that place is flowing towards that place, the water rises there. If the water on one side is flowing rapidly towards it, and the water on the other side is receding slowly from it, the water rises there. When the surface at any one place is stationary as to height, there may nevertheless be considerable horizontal velocity; only it is certain that the water is flowing towards it on one side exactly as fast as it is receding from it on the other side.

Now in this third diagram let the strong elliptic outline represent the form of the surface of the water at the present instant; and let the dotted line represent the form which it will take in a short time; the form of the dotted curve being the same as that of the strong line curve, but having turned round with the same angular velocity as the Moon.

At A, C, E, and G, the height of the water has scarcely altered from the state of things with the strong outline to the state of things with the dotted outline. Therefore, the speed of the water is equal on both sides of each of these four points. And therefore it will readily be understood from the ordinary theory of maxima and minima that at these four points the horizontal motion of the water is most rapid: its direction at each being at present undecided.



At B and F the water is rising most rapidly; therefore the water is flowing from both sides towards B and towards F.

At D and H the water is sinking most rapidly; therefore the water is receding on both sides from D and from H.

Hence it follows that the directions of currents are represented by the arrows in the diagram.

We have now obtained complete knowledge of the state of the currents in the strong-line ellipse. And from these we can infer the state of the currents in the dotted-line or subsequent ellipse, remarking that in this subsequent case the subsequent currents maintain the same relation to the axes of the subsequent ellipse which in the preceding case the preceding currents held to the axes of the preceding ellipse. And, by comparing these, we shall learn what are the changes made in the currents at each place, and what must be the forces which produce these changes.

In this fourth diagram—

At A, C, E, G, the current is scarcely changed, or the forces are o.

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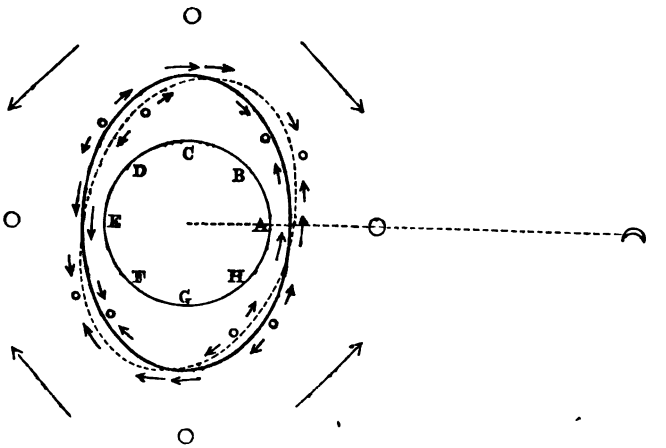
At B a current o is changed to ↘, and a current ↖ is changed to o, or the force is ↘.

At D a current o is changed to ↙, and a current ↗ is changed to o, or the force is ↙.

In like manner, at F, the force is ↖; and at H the force is ↗.



These forces are such as are produced by the Moon in the position shown in the diagram, or in the opposite position; and in no other position. Therefore *it is low water under the Moon.*

In this investigation it is to be remarked that I have not yet taken account of the pressure produced by the head of water at C and G. Its tendency is exactly the same as that of the lunar forces, namely, to push the water both from C and from G towards both A and E. It therefore does not alter the law of tides, but merely requires a smaller agency of the Moon to produce a tide of given magnitude. In other words, the tide is rendered larger by this consideration.

Appendix.

Since presenting this paper to the Secretary of the Society, I have endeavoured to extend the investigation to higher powers of small quantities. As far as applies to another power of p (the quotient of Earth's radius by Moon's distance) there is no difficulty; the dragging force of the Moon on the waters is represented by H × { sin 2 hour-angle + $\frac{1}{4}p$. sin hour-angle + $\frac{5}{4}p$. sin 3 hour-angle}; the first and second of these terms

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produce terms in the elevation of the water with f for factor, and depending on sin 2 hour-angle and sin hour-angle; and these terms when multiplied by (p . sin hour-angle + $\frac{3}{2}p^2$ sin 2 hour-angle) to give the amount of force accelerating the Moon in her orbit (and therefore retarding her mean longitude) give two additive constant terms. The term resulting from the product of the term of elevation depending on sin . hour-angle by the factor p . sin hour-angle, appears to me to correspond to the displacement of the centre of gravity of the united mass of nucleus and water, and therefore it appears to produce no effect in the Moon's motion round the centre of gravity. But there is no displacement of the centre of gravity corresponding to the terms depending on sin . 2 hour-angle; and those terms appear to me to produce a real effect on the Moon's motion.

The terms introduced by the higher power of p do not produce any expression of frictional retardation in the rotation of the Earth's nucleus.

As far as applies to a higher power of the force, or (which in the investigation becomes equivalent to it) a higher power of "tidal elevation divided by depth of sea," the following investigation is given, rather for the purpose of showing how the effect of the finite proportion of tidal rise to the depth of sea may mathematically be taken into account, than for the general conclusion, which may be obtained by simple reasoning. The introduction of the higher powers of p produces great complexity and adds nothing to the value of the result, and I shall therefore omit them.

The friction between the water and the nucleus, as affecting the motion of the water, has been expressed, as an accelerating force, by the symbol $-f\frac{dX}{dt}$. Its expression as a moving force, on the quantity of fluid which originally occupied the space between x and $x + \delta x$, will be $-f\frac{dX}{dt} \times k\,\delta x$. The summation of this for the whole channel (which, with sign changed, gives the action upon the nucleus) will require us simply to integrate $-fk\frac{dX}{dt}$ with respect to x. It is only necessary therefore to examine whether $\frac{dX}{dt}$ can contain any non-periodic term.

The depth of water is expressed by

$$\frac{k}{1+\frac{dX}{dx}} \text{ or } k\left\{1-\frac{dX}{dx}+\left(\frac{dX}{dx}\right)^2\right\}.$$

The tidal elevation, for the part whose original ordinate was x, is $k\left\{-\frac{dX}{dx}+\left(\frac{dX}{dx}\right)^2\right\}$. That for the part whose original

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ordinate was $x + h$, is

$$k\left\{-\frac{d\mathrm{X}}{dx}-\frac{d^2\mathrm{X}}{d^2x}h+\left(\frac{d\mathrm{X}}{dx}\right)^2+2\frac{d\mathrm{X}}{dx}\cdot\frac{d^2\mathrm{X}}{dx^2}\cdot h\right\}.$$

The excess of the former is $kh\left\{\frac{d^2\mathrm{X}}{dx^2}-2\frac{d\mathrm{X}}{dx}\cdot\frac{d^2\mathrm{X}}{dx^2}\right\}$. The pressure of this head of water acts on the fluid through the whole depth; and its entire pressure is therefore, whole depth $\times kh\left\{\frac{d^2\mathrm{X}}{dx^2}-2\frac{d\mathrm{X}}{dx}\cdot\frac{d^2\mathrm{X}}{dx^2}\right\}$. The mass of water to be moved is kh. Therefore the accelerating force is,

$$g\times\text{whole depth}\times\left\{\frac{d^2\mathrm{X}}{dx^2}-2\frac{d\mathrm{X}}{dx}\cdot\frac{d^2\mathrm{X}}{dx^2}\right\}$$

$$=gk\left\{\frac{d^2\mathrm{X}}{dx^2}-3\frac{d\mathrm{X}}{dx}\cdot\frac{d^2\mathrm{X}}{dx^2}\right\}.$$

Hence, the equation of motion will become,

$$0=-\frac{d^2\mathrm{X}}{dt^2}+\mathrm{H}\cdot\sin(it-mx)-f\frac{d\mathrm{X}}{dt}+gk\frac{d^2\mathrm{X}}{dx^2}-3gk\frac{d\mathrm{X}}{dx}\cdot\frac{d^2\mathrm{X}}{dx^2};$$

in which the last term is considered as small.

It does not appear practicable to solve this equation except by successive substitution; first solving the equation without the last term, then substituting in the last term the value so found for X, and thus producing new functions of t and x to be annexed to $\mathrm{H}\cdot\sin(it-mx)$, by which a new equation will be prepared for solution in the same manner as the first. Taking then $\mathrm{X}=\mathrm{A}\cdot\sin(it-mx)+\mathrm{B}\cdot\cos(it-mx)$, where A and B have the determinate values already found:

$$\frac{d\mathrm{X}}{dx}=-m\,\mathrm{A}\cdot\cos(it-mx)+m\,\mathrm{B}\cdot\sin(it-mx);$$

$$\frac{d^2\mathrm{X}}{dx^2}=-m^2\mathrm{A}\cdot\sin(it-mx)-m^2\mathrm{B}\cdot\cos(it-mx),$$

their product is

$$\tfrac{1}{2}m^3(\mathrm{A}^2-\mathrm{B}^2)\cdot\sin(2it-2mx)+m^3\mathrm{A}\,\mathrm{B}\cdot\cos(2it-2mx),$$

and the equation of motion now is,

$$0=-\frac{d^2\mathrm{X}}{dt^2}+gk\frac{d^2\mathrm{X}}{dx^2}-f\frac{d\mathrm{X}}{dt}+\mathrm{H}\cdot\sin(it-mx)-\frac{3}{2}gkm^3(\mathrm{A}^2-\mathrm{B}^2)\cdot\sin(2it-2mx)$$

$$-3gkm^3\;\mathrm{A}\;\mathrm{B}\;\cdot\cos(2it-2mx).$$

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On solving this in the same manner as before, it is evident that the expression for X will contain only sines and cosines of $it - mx$ and $2it - 2mx$, and therefore $\frac{dX}{dt}$ will contain only similar terms; and the integral of $fk \cdot \frac{dX}{dt}$ through the whole circumference = 0. This, with sign changed, is the moving force tending to retard the sphere's rotation; and therefore, to the second order of disturbing forces inclusive, the friction of the tides does not tend to retard the rotation of the earth's nucleus.

But the aggregate effect, as obtained here, may also be found from the following simple reasoning:—If the friction vary as the velocity, the whole force impressed on the nucleus by a given portion of the water in passing through a certain space is exactly proportional to that space, and an exactly equal force will be impressed in the opposite direction by returning through the same space; and thus, in the most complicated case of oscillating motion, if at any time the particles return to the same position as at first, the frictional effect is 0.

If the force vary as the square of the velocity, the difficulties of mathematical treatment appear to be almost insuperable; the frictional function being discontinuous, changing its sign whenever the complicated function which expresses the velocity becomes 0. We can, however, assert that if the velocity in the + direction and that in the — direction follow the same laws, the aggregate frictional effect will be 0. This will certainly hold if the terms of the second order of p be omitted; but it seems doubtful whether it will hold if they be retained.

1866, *April* 3.

Addendum.

1866, *April* 5.

I have at length discovered two terms which appear to exercise a real effect on the rotation of the Earth.

First. The coefficient of horizontal forces, which I have designated by H, is not constant, but is proportional to the distance from the Earth's centre. If we take that value which corresponds to half the depth of the water, the true value or H′ may be represented nearly by

$$H \times \left\{ 1 + \frac{1}{2r} \text{ tidal rise} \right\}$$

$$\text{or } H \times \left\{ 1 - \frac{1}{2r} \cdot \frac{m H k}{\sqrt{\{(i^2 - g k m^2)^2 + f^2 i^2\}}} \cos (it - mx + F) \right\}$$

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and the true horizontal force, or $+ H' . \sin(i t - m x)$, will be

$$H . \sin(i t - m x)$$

$$- \frac{m H^2 k}{4 r \sqrt{\{(i^2 - g k m^2)^2 + f^2 i^2\}}} \times \{ \sin(2 i t - m x + F) - \sin F \}.$$

Giving to sin F its value, this contains the constant term

$$+ \frac{m H^2 k f i}{4 r \{(i^2 - g k m^2)^2 + f^2 i^2\}}.$$

Second. The ordinate of the water on which the force is acting is not x but $x + X$; and therefore, instead of using the expression $H . \sin(i t - m x)$, we ought to use

$$H . \sin(i t - m x - m X),$$

or

$$H . \sin(i t - m x) - m H X . \cos(i t - m x).$$

Putting for X its value, the last term becomes

$$+ \frac{m H^2}{\sqrt{\{(i^2 - g k m^2)^2 + f^2 i^2\}}} . \sin(i t - m x + F) . \cos(i t - m x);$$

which, with similar expansion, and substitution for F, gives the constant term $+ \frac{m H^2 f i}{2\{(i^2 - g k m^2)^2 + f^2 i^2\}}$. This term is much larger than that found above.

Call the sum of these two terms $+ c$. Then the equation of motion, as applying to this term only, and putting b^2 for $g k$, becomes $\frac{d^2 X}{d t^2} - b^2 . \frac{d^2 X}{d x^2} = + c$.

Let $b t + x = u$, $b t - x = v$; then this equation may be changed into

$$4 b^2 \frac{d^2 X}{d u . d v} = + c; \frac{d X}{d v} = + \frac{c v}{4 b^2} + \varphi'(v); X = + \frac{c u v}{4 b^2} + \varphi(v) + \psi(u).$$

$$\text{Or,} \quad X = \frac{c}{4 b^2}(b^2 t^2 - x^2) + \varphi(b t - x) + \psi(b t + x).$$

For determining the form of the arbitrary functions, we remark that the final solution must contain no power of x (otherwise we should have inconsistent values in completing the round of the circle), and that the form of the first term

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then admits only algebraical functions. Thus we find that the solution must be,

$$X = + \frac{c}{4b^2}(b^2t^2 - x^2) + \frac{c}{8b^2}(bt - x)^2 + \frac{c}{8b^2}(bt + x)^2 = + \frac{c}{2}t^2;$$

$$\frac{dX}{dt} = + ct.$$

That is, there is a constant acceleration of the waters as following the Moon's apparent diurnal course. As this is opposite to the direction of the Earth's rotation, it follows that from the action of the Moon there is a constant retarding force on the rotation of the water, and therefore (by virtue of the friction between them) a constant retarding force on the rotation of the Earth's nucleus.

If, as in the preceding cases, we include in the equation the term depending on f, the form of solution is somewhat modified, and a better view of the effect of the new term is obtained. Remarking that, as is stated above, no powers of x can be admitted, we may omit $\frac{d^2X}{dx^2}$; and the equation, as regards the new term, becomes,

$$\frac{d^2X}{dt^2} + f\frac{dX}{dt} = + c.$$

The solution of this equation is

$$X = \frac{ct}{f} + c' + c'' \cdot \epsilon^{-ft};$$

from which the friction

$$= -f\frac{dX}{dt} = -c + f^2 \cdot c'' \cdot \epsilon^{-ft}.$$

Whatever, therefore, be the primary state of things, the second term will ultimately become insensible; the frictional force on the water will be $-c$ in the direction of the Moon's apparent diurnal motion, or $+c$ in the direction of the Earth's rotation; and this implies a force $-c$ in that direction upon the nucleus of the Earth, constantly retarding its rotation.

I am very happy to give my entire assent to the general views of M. Delaunay on the existence of one real cause for the retardation of the Earth's rotation.

G. B. Airy.

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On a Method of Computing Interpolations to the Second Order without Changes of Algebraic Sign. By G. B. Airy, Astronomer Royal.

In the year 1847 I suggested (in a paper printed in the sixteenth volume of the *Memoirs* of this Society) a form of computation by which the troublesome changes of sign in Bessel's method of computing star-reductions might be completely avoided.

Using capital letters for numbers depending on the day, and small letters for numbers depending on the star's place, it is known that the star-reductions in R.A. and N.P.D. are expressed by formulæ of this character:—

$$\mathrm{A}a + \mathrm{B}b + \mathrm{C}c + \mathrm{D}d;$$
$$\mathrm{A}a' + \mathrm{B}b' + \mathrm{C}c' + \mathrm{D}d';$$

in which formulæ every one of the twelve symbols, of their eight products, and of the two aggregates, may be positive or negative.

For the numbers A, a, a', &c., I suggested a series of numbers E, e, e', &c., by use of which the star-reductions in R.A. and N.P.D. are expressed by the following formulæ:—

$$\mathrm{E}e + \mathrm{F}f + \mathrm{G}g + \mathrm{H}h + \mathrm{L} + l - 300^{s}{\cdot}000$$
$$\mathrm{E}e' + \mathrm{F}f' + \mathrm{G}g' + \mathrm{H}h' + \mathrm{L} + l' - 300''{\cdot}000$$

in which formulæ every one of the fifteen symbols, of the eight products, and of the aggregates preceding the number $-300{\cdot}000$, is essentially positive; the only exception being in the numbers e, f, g, h, l, for a very few stars near the pole (not exceeding in number one in a hundred and fifty for an ordinary general catalogue of stars), which sometimes make one of the five symbols negative.

This system was soon brought into use at the Royal Observatory, and with perfect success. The amount of mental effort required by the computers of lower class who usually make the calculations, and by the assistants of higher class who examine them, is most materially diminished. The most fertile source of errors is at once taken away.

The success of this system for star-reductions, confirmed by the experience of many years, induced me to attempt a similar change in the form of interpolations with second differences; a subject of great importance in the calculations at the Royal Observatory. Our annual interpolations are more than three thousand in number; and, since I took the superintendence of the Observatory, I believe that the whole number has approached to eighty thousand. Every one of these (before

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making the change which I am about to describe) was liable to the uncertainties of sign in the first power of the time, in the first difference, in the second difference, in the numbers which they respectively produce, and in the steps of their aggregation with the ephemeridal quantity; and experience had shown that the greater number of errors in result arose from errors in these signs.

I shall now explain, first, the process which had been long used at the Observatory, and secondly, the steps of improvement by which it was changed to the form finally adopted.

If we take from the Ephemeris the values x_1, x_2, x_3, corresponding to the equidistant times T_1, T_2, T_3; and if we wish to compute the value of x for the time $T_2 + \tau$ (where τ is expressed by a number whose unit is $T_2 - T_1$ or $T_3 - T_2$, and is always less than $\pm$ 1, and practically always less than $\pm \frac{1}{2}$), we begin by taking the differences, thus:—

		First Differences.	Second Difference.
T_1	x_1		
		$\Delta'(1)$	
T_2	x_2		$\Delta(2)$
		$\Delta''(1)$	
T_3	x_3		

Then, forming the coefficients $\frac{1}{2}\{\Delta'(1) + \Delta''(1)\}$ and $\frac{1}{2}\Delta(2)$ numerically, the required quantity x is

$$x_2 + \frac{1}{2}\{\Delta'(1) + \Delta''(1)\} \times \tau + \frac{1}{2}\Delta(2) \times \tau^2.$$

This formula is very conveniently adopted to logarithmic computation; but is subject to the inconvenience of change of sign in nearly every element.

The first step of improvement may be made by referring the time for computation, not to the second ephemeridal time T_2, but to the first ephemeridal time T_1. Thus, while we always adopt, for the middle ephemeridal time T_2, that ephemeridal time which is nearest to the time for which the interpolation is to be made, let us refer the interpolation time to T_1; or, let us call the interpolation time $T_1 + t$. The value of t (estimated in the same manner as that of τ) will always be included between $+\frac{1}{2}$ and $+\frac{3}{2}$, and will, therefore, be essentially positive. The formulæ of interpolation will now be

$$x = x_1 + \{\Delta'(1) - \frac{1}{2}\Delta(2)\} \times t + \frac{1}{2}\Delta(2) \times t^2.$$

The factors t and t^2 are now positive; but the coefficients $\Delta'(1)$ and $\Delta(2)$, as before, still present in an unusual degree the practical troubles attending changes of sign; they are formed sometimes by subtracting a lower number from an

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upper; sometimes the difference has the same sign as the number differenced, and sometimes a different sign; and there are the usual troubles attending the signs of the coefficients in the interpolation-formula, and the aggregation of the terms.

In order to remove all the troubles attending the differences, an examination was made of the elements for which we have usually to interpolate. Those subject to the greatest changes are the Moon's R.A. (in seconds of time, and given for every hour), the Moon's N.P.D. (in seconds of arc, and given for every hour), and the Moon's Parallax and Semidiameter (in seconds of arc, and given for every twelve hours). (The first difference of the Moon's R.A. is never negative; but, for uniformity of system, it was included under the same form of calculation as the others.) On examination, it was found that for N.P.D. the greatest negative first difference never amounted to $-20'$, and the greatest negative second difference never to -1; that for Parallax and Semidiameter, the greatest negative first difference never amounted to $-40''$, and the greatest negative second difference never amounted to $-10''$; and that for R.A. the greatest negative second difference never amounted to -10^s. Therefore, if we prepare the following quantities (which, in a proper skeleton form, is a very simple process, and involves no change of sign and no subtraction):—

For Moon's R.A.	For Moon's N.P.D.	For Moon's Parallax and Semidiameter.
x_1	x_1	x_1
$x_2 + 40^s$	$x_2 + 20'$	$x_2 + 40''$
$x_3 + 90^s$	$x_3 + 41'$	$x_3 + 90''$;

and if we then take their first and second differences, every operation of forming differences is a subtraction of an upper number from a lower; every number so formed is positive; the coefficient $\Delta'(1) - \frac{1}{2}\Delta(2)$ is always formed in the same manner; every number to be substituted in the last formula of interpolation

$$x_1 + \left\{\Delta'(1) - \frac{1}{2}\Delta(2)\right\} \times t + \frac{1}{2}\Delta(2) \times t^2$$

is positive; and every operation of connexion of results is a numerical addition.

But, in this process, we have introduced into our final sum a quantity which depends upon our additive numbers (40 and 90, or 20 and 41), which quantity must now be subtracted. Taking the differences of these numbers,

0				0		
	40				20	
40		10	or	20		1
	50				21	
90				41		

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it will be seen that we have introduced the quantity

$$0 + 35 \times t + 5 \times t^2 \quad \text{or} \quad 0 + \frac{39}{2} \times t + \frac{1}{2} \times t^2.$$

Tables of these quantities (called Table T* and Table T**) are computed with the argument t; and, from one of these tables, T* or T** is taken (according to the series of additive constants which has been used) and is applied, in all cases subtractively, to the number produced by the interpolation-formula.

It will be remarked that the numbers added for R.A. are the same numerically (though in different denominations) as those for Parallax and Semidiameter, and, therefore, the same table T* is applicable to those three quantities. But the tabular intervals are different, being 1^h for the R.A. and 12^h for the Parallax and Semidiameter. This requires a modification of the arguments. The first and last numbers of Table T* are as follows:—

t. For R.A. m s	For Parallax and Semid. h m s	T*.	t. For R.A. m s	For Parallax and Semid. h m s	T*.
30 0	VI. 0 0	18·750	89 55	XVII. 59 0	63·681
1	12	18·761	56	12	63·695
2	24	18·772	57	24	63·709
3	36	18·783	58	36	63·722
4	48	18·794	59	48	63·736
5	1 0	18·806	90 0	XVIII. 0 0	63·750

For T**, which is used for North Polar Distance only, a single column of arguments suffices. The first and last numbers of Table T** are as follows:—

t. For N.P.D. m s	T**.	t. For N.P.D. m s	T**.
30 0	9 52·500	89 55	30 20·750
1	9 52·833	56	30 21·000
2	9 53·167	57	30 21·450
3	9 53·500	58	30 21·800
4	9 53·833	59	30 22·150
5	9 54·167	90 0	30 22·500

This system of interpolation has now been exclusively used at the Royal Observatory from the beginning of 1862.

Royal Observatory, Greenwich,
1866, *March* 27.

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The New Star of the Year 393 A C.: Documents taken from the Chinese Observations of Ma-tuan-lin, translated by Edouard Biot, and the Remarks of M. Humboldt in his Cosmos.

(*Communicated by M. Hermann Goldschmidt.*)

It is a long time since I attempted to prove that the new stars of the years 393, 827, 1203, and 1609, are one and the same. I was struck by the fact that the three first stars were seen in the same constellation of *Scorpii*. That of 393 was visible in the month of March near the star μ^2. The new star of the year 1203 was exactly on the same place of the sky at the end of July.

The interval of time between these two apparitions are 810 years and a few months, and the half of this is 405 years and 70 days. If this epoch represents the duration of a period, the star should have come back in the year 798. The Arabic astronomers mentioned a new star on the 15th degree of *Scorpii*, in the year 827, but the date is given as doubtful. It is said that in the year 827, this star was observed under the reign of the Calife Al-Mamoun, who governed between the years 814–833. On taking for exact a period of 405 years, the star should have returned again to its visibility at the end of the year 1608, or at the beginning of 1609; and, indeed, M. Biot found mentioned a brilliant star in the year 1609, in the Chinese collection of Ma-tuan-lin. The historian only says that the star was seen on the south-west of the firmament, but it is essential that a new star appeared in that year. New stars are not so frequent as to exclude this apparition, which is ranging itself with the period of 405 years.

These considerations are sufficient, I think, to show the probability of a period. The year 827 is alone given as doubtful by M. de Humboldt in his *Cosmos*, remarking that the apparition is to be put rather in the first half of the ninth century; but we learn that the star was observed by the celebrated Babylonian astronomers, Hali and Giafar Ben-Mohammed Alboumazar, under the reign of the Calife Al-Mamoun. This prince was himself an astronomer, and this science flourished under his reign. Considering these facts, the year of the apparition cannot be so doubtful as to give a latitude of half a century.

The anomaly of this star, which was seen in the year 827, I suppose, instead of 798, or twenty-nine years later, cannot raise any serious doubt of its identity with the other ones. Can we not find any analogy with some variable star? I have observed U *Geminorum* (Hind No. 6) coming exceptionally back 24 days before its ordinary return, and this with a period of 96 days. It remains scarcely 12 days visible, and the greatest light is for 2–3 days duration. The apparition of

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29 years later for the new star, can also be regarded as an irregularity exceptional, for the other apparitions concord with the period assigned, and it is not probable that the star of 1609 was another one. For U *Geminorum* we count days of its irregularity, and for the new star of 827, years. We have then

1.	Apparition of the year	393	March.
2.	... ...	798	
	... ...	827	
3.	... ...	1203	End of July.
4.	... ...	1609	
5.	Reapparition probable	2014–2015	

It is remarkable that 134 years B.C. a new star was seen in *Scorpius*, which it is believed gave to Hipparchus the idea of laying down a catalogue of stars. In China there was also seen in the year 1584 a star near π *Scorpii*.

Fontainebleau, March 2, 1866.

On a Probable Observation of Biela's Comet.

By C. G. Talmage, Esq.

In communicating this observation to the Society, it is, I think, necessary to enter somewhat into particulars. At our last meeting I was publicly asked whether I could give any information respecting Biela's Comet. I then stated that I could not *positively* give an observation, but that, while sweeping for Biela's Comet, on the 4th of November last, I came upon a nebulous object that I think is very likely to have been the Comet, and I here give extracts from my letters to my friend Mr. Hind on the subject.

"1865, *November* 9.

". . . On the 4th, between a break in the clouds, I got a glimpse of a cometic-looking object, the instrumental position of which was

R.A.		22 55 4
Declination ...		+ 13 25 55
Local Sid. Time ...	=	0 20 12·07

Clouds closed up so quickly that I could obtain no comparison

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with any star; it was exceedingly faint; every night since has been exceedingly dark."

"1865, *Nov.* 17.

"I have been waiting to get a sight of my object of the 4th, but we have had dreadful weather, not a star visible all night.

"These are the first observations since the return of our hour-circle (hours given below)."

From Mr. Hind to myself:

"*Richmond*, 1865, *Nov.* 20.

"Pray look with all your eyes for Biela. I think in all probability your object of the 4th was really nucleus L. Mr. Barber caught a glimpse on the 18th of a cometary object nearly on the declination of my ephemeris for Jan. 27·66, and following something less than a minute. I expect this is about the most likely position for it to turn up."

I may also give an extract from a letter of Mr. Hind, dated 1865, Nov. 8, which crossed mine in the post:

"On Sunday I had a close search here, and certainly suspected a nebulosity not far from the place with P.P. Jan. 27, but could not decide about it; only clear for twenty minutes."

An exact copy from my observing-book:

1865, November 4.

Hour Circle	...	h m s 13 25 8	∴	h m s 1 25 8 W.
Declination Circle	...	° ′ ″ +13 25 55		Corrected for refraction.
Sidereal Time	...	h m s 0 20 30		Clock Error = + 17·93 s.
G.M. Time	...	9 23 42·30		
∴ at	...	0 20 12·07		h m s R.A. = 22 55 4·07
				Dec. = +13 25 55

On the 16th, the first fine night after the 4th, the index-errors were determined by *α Pegasi* and *ζ Pegasi* to be

Error of Hour-circle by *α Pegasi* = + 18·53 s

,, ,, *ζ Pegasi* = + 18·67

Adopted Error = + 18·60 s.

And the Adopted Error of the Declination Circle = − 2″·6

which reduces the observation to

Leyton Sidereal Time. h m s	R.A. of Object. h m s	Declination of Object. ° ′ ″	G.M.T. h m s
0 20 12·07	22 54 45·47	+ 13 26 21	9 23 42·30

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Compared with Mr. Hind's Ephemeris, with P.P. of Jan. 27·66, the errors are

$$\Delta\alpha = -\,26^{s}\cdot07 \qquad \Delta\delta = -\,1'\,2''$$

Mr. Barclay's Observatory, Leyton.

Occultation of 31 *Arietis*, 1866, *March* 19.
By C. G. Talmage, Esq.

G.M.T. of disappearance = 7 h 54 m 25·26 s
,, reappearance = 8 15 30·79

At the disappearance on the dark limb, the star was projected on the Moon's disk for a very appreciable time; the limb of the Moon being distinctly visible on the following side of the star. The reappearance is considered good, several small stars were occulted during the time the star (31 *Arietis*) was hidden by the Moon.

Power 80 on the 10-inch refractor.

On the Companion to Sirius. By George Knott, Esq.

Happening to turn my 7¼-inch Alvan Clarke refractor on *Sirius*, on the 24th of January, I was surprised to find the small companion, notwithstanding bright moonlight, a tolerably conspicuous object. Its colour was a fine pale blue (about Blue[3] of the late Admiral Smyth's chromatic scale), and it bore a sufficiently strong illumination to allow of my measuring it in position and distance with a wire-micrometer, mag. power 375, with the following results :—

P = 77°·21 obs. 5, w. 22; D = 10″·433 obs. 4, w. 10; Epoch 1866·064.

On the 2nd of February, although the atmosphere was too disturbed to allow of distance-measures being taken, I again succeeded in measuring with the wire-micrometer and power 275, the angle of position of the companion as follows :—

P = 77°·04 obs. 5, w. 10; D impossible; Epoch 1866·088.

Combining the two sets, allowing weights, we have the following mean results :—

P = 77°·16 obs. 10, w. 32; D = 10″·433 obs. 4, w. 10; Epoch 1866·07.

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Of course, in the case of a pair so difficult to measure, especially in these latitudes, these results are open to some uncertainty, but they will be found to present a fair accordance with the predictions of Dr. Auwers' Ephemeris, printed in the *Monthly Notices* for December 1864, the calculated co-ordinates for 1866·0 being P = 78°·50, D = 10″·75. I do not know that much importance is to be attached to the circumstance, but it may perhaps be worthy of a passing remark that, with the single exception of the measures of the Rev. W. R. Dawes in 1864, all the *observed* angles of position quoted by D. Auwers (and my own result may now be added to the list) are *less* than the *calculated* angles.

I had hoped to have succeeded in obtaining further measures of this interesting pair, but have hitherto been prevented from doing so by unfavourable weather. I may just remark, in conclusion, that my measures on February 2 were taken with the aperture of the telescope reduced to 6½ inches, and that on that occasion I found that the small star was still visible by glimpses with 5 inches of aperture, and I am quite inclined to indorse the opinion expressed by Mr. Dawes that the visibility of the small star is dependent rather on the condition of the atmosphere than on the size of the telescope.

Woodcroft Observatory, Cuckfield,
April 12, 1866.

Companion of Antares. By Arthur Cottam, Esq. F.R.A.S.

With reference to the last part of Mr. Freeman's note in the last *Monthly Notice*, I may mention that on two evenings at the end of last summer I saw the Companion without any difficulty with a 4½-inch object-glass. I have not since been able to observe *Antares*, but, on both the evenings referred to, the Companion was at times clearly separated from the blaze of the large star, and certainly appeared to be of a green colour. On the second evening my brother, who happened to be with me, saw the Companion also. On both occasions the star was at least an hour west of the meridian, and the atmosphere not particularly good. The powers used were 150 and 200.

My Observatory here stands at an elevation of 500 feet above the sea level, and it is very rarely that I am prevented by fog from making observations.

As I supposed that a 4½-inch object-glass ought to show the companion of *Antares*, I have not before thought this worth communicating to the Society, but after seeing Mr. Freeman's note I am inclined to think that my position must be a good one.

Bushey Heath, Watford, Herts,
April 12, 1866.

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On a Small Star near ε Canis Majoris.

By D. A. Freeman, Esq.

I beg leave to state that I have observed a small star near to ε *Canis Majoris*. It is *s*, a little *f*. On comparing its distance from ε with the distance between *Rigel* and its Companion, I estimate that its distance is rather less than the latter, and that its magnitude is 12 or 13. Owing to the low altitude of ε, the small star is difficult to observe with my aperture of 4½-inch, and I did not feel quite certain of its existence until yesterday evening, when the very fine atmosphere, which enabled me to see the 5th and 6th stars in the trapezium with equal facility, dispelled all doubt about it.

I am not aware whether this small star has been the subject of observation, and its position and distance measured. There are other minute and faint stars at a greater distance, *n.p.* the large star.

Mentone, Alpes Maritimes,
23 March, 1866.

The French Academy of Sciences have awarded the Lalande Prize for Astronomy to Mr. Warren De La Rue, for his Works in Celestial Photography. The Report of the Commission, consisting of MM. Mathieu, Delaunay, Liouville, Faye, and Laugier, drawn up by M. Laugier, and published in the *Comptes Rendus*, March 5, 1866, is as follows:—

"Il y a bientôt vingt-sept ans que la decouverte de Daguerre est connue, admirée et exploitée dans le monde entier. Grâce à un grand nombre de travaux distingués, d'importants perfectionnements ont été réalisés, et celle belle invention a fini par donner naissance en quelque sorte à une nouvelle branche d'industrie. Les sciences d'observation, l'Astronomie entre autres, n'ont pas tardé à lui devoir de notables progrès. Nous n'entreprendrons pas d'exposer dans ce Rapport les titres des astronomes et des physiciens qui ont contribué à ces progrès: seulement nous allons faire connaître en quelques mots ceux qui ont signalé l'un d'eux, M. Warren De La Rue, au choix de la commission du prix Lalande.

"Il y a dixhuit ans que M. Warren De La Rue a établi son observatoire privé à Cranford, près de Londres, et depuis quinze ans environ il s'est spécialement livré à l'etude de la photographie céleste. L'instrument qu'il a employé dans ses laborieuses et délicates recherches est un télescope de 13 pouces anglais d'ouverture, monté sur un pied parallactique mû par une horloge et construit sous sa direction d'après ses dessins. Les belles photographies lunaires que M. De La Rue a fait connaître au monde savant prouvent le degré de perfection de

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son grand appareil sous le double rapport optique et mécanique. A l'aide du mécanisme d'horlogerie il peut modifier le mouvement de sa lunette et lui faire suivre exactement les variations de la vitesse de la Lune. En perfectionnant les procédés chimiques employés pour la préparation de la surface sensible, il est parvenu à réduire notablement la durée d'exposition de cette surface à l'action des rayons lumineux. Enfin tour à tour opticien, mécanicien, chimiste, et astronome, M. De La Rue a eu la satisfaction de voir ses efforts couronnés de succès. Les images photographiques de la Lune qu'il a obtenues a diverse reprises sont d'une perfection telle qu'elles peuvent supporter l'amplification considérable de 36 pouces anglais en diamètre: et elles se prêtent à des mesures micrométriques si exactes qu'elles ont fourni des données précises pour la mesure de la libration.

"Dans les séances memorables de l'Académie des Sciences où Arago rendit compte des procédés de Daguerre, il énumérait les applications que l'Astronomie pourrait en faire un jour, et déjà, d'après la première épreuve de la Lune que Daguerre avait obtenue sur sa demande, il predisait qu'on ferait des chartes photographiées de notre satellite. Cette prévision se réalise en ce moment: les belles épreuves de M. De La Rue sont employées comme fondements de la grande carte de la Lune de 6 pieds anglais de diamètre, entreprise sous les auspices et d'après les ordres de l'Association Britanique pour l'avancement des sciences. Il est parvenu à produire des vues stéreoscopique lunaires qui peuvent faire connaître exactement les hauteurs et les dépressions relatives des ravins, plateaux, et ondulations dont la surface de la Lune paraît sillonée. Ajoutons que M. De La Rue a également obtenu des épreuves photographiques de *Saturne*, de *Jupiter*, de *Mars*, et de quelques étoiles.

"Ces beaux travaux de photographie céleste suggérèrent en Angleterre l'idée d'établir à l'Observatoire de Kew un instrument spécial, et ce fut naturellement à M. De La Rue qu'on en confia la direction. Depuis 1858 qu'il est installé, l'appareil héliographique de Kew a donné plusieurs résultats importants. Lors de l'éclipse totale du Soleil en 1860 il fut emporté en Espagne par M. De La Rue à la demande de la Société Royale Astronomique, et cette habile astronome put prendre une série d'épreuves de l'eclipse, avant, pendant, et après disparition totale du Soleil. Des mesures effectuées sur ces épreuves à l'aide d'un micromètre inventé par M. De La Rue furent soumises au calcul, et la discussion montra que les changements angulaires des proéminences lumineuses par rapport à la Lune s'accordent avec l'hypothèse de leur adhérence au Soleil; que l'apparence des flammes ne varie qu'en raison du deplacement de la Lune, et ne soubit aucune autre alteration; de sorte que lorsque le mécanisme d'horlogerie est ajusté sur le mouvement du Soleil, les flammes paraissent immobiles. Enfin la comparaison des

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photographies de l'éclipse obtenues à Rivabellosa, ou observait M. De La Rue, avec celles que le P. Secchi obtint au Desierto de las Palmas, montra l'identité des proéminences observées aux deux stations, en tenant compte bien entendu des effets de la parallaxe dus à la différence des stations ; établissant ainsi qu'aucune modification des proéminences pendant un laps de temps beaucoup plus long que la durée de l'éclipse totale, puisque, dans ces deux stations, le phénomène se produisit à un intervalle de sept minutes.

"En 1859 M. De La Rue obtint des vues stéreoscopiques du Soleil, en profitant de son mouvement de rotation sur son axe, et ces vues de taches et de facules permettent d'étudier les positions rélatives des parties qui composent la photosphère. Il a montré également la possibilité d'obtenir par l'action de la lumière seule, des plaques pouvant imprimer avec les encres ordinaires d'imprimerie les épreuves photographiques de la Lune et du Soleil.

"Depuis 1863 l'appareil héliographique de l'Observatoire de Kew fonctionne sans interruption, et les épreuves quotidiennes sont relevées et discutées sous la direction de M. De La Rue. Enfin sur la demande de la Gouvernement Russe un second appareil du même genre a été établi à Wilna, et le directeur de cet observatoire a reçu de M. De La Rue toutes les instructions nécessaires pour en faire usage.

"*Conclusions.*

"La Commission propose à l'Academie d'accorder à M. Warren De La Rue, pour l'ensemble de ses beaux travaux de photographie céleste, le prix d'Astronomie de la fondation Lalande."

Instrument for Sale.

To be disposed of, a 32-inch Transit Instrument, made by Simms, London, mounted on stone pillars; $2\frac{1}{4}$-inch object-glass; declination and micrometer circles, divided on silver collimator; three eye-pieces, 37, 66, 100; prism for zenith observations; stride level, &c. &c. For further particulars inquire of J. Jenkins, Esq., Rotherslade, near Swansea.

With this Number are issued plates to illustrate Mr. Howlett's paper on Sun-spots, printed in the first number of the present volume, p. 13.

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University of MICHIGAN

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *May* 11, 1866. No. 7.

WARREN DE LA RUE, Esq., Vice-President, in the Chair.

Henry Boys, Esq., Cambridge;
Samuel Hunter, Esq., Kingstown, Ireland;
Josiah Thomas Slugg, Esq., Manchester;
Edward Carlton Tuffnel, Esq., 26 Lowndes Square;
Rev. H. Storer Toms, Enfield;
Williams Nutter Barker, Esq., Enfield;
John Smith, Esq., Dorking; and
J. E. Saunders, Esq., 9 Finsbury Circus,

were balloted for and duly elected Fellows of the Society.

Dr. Auwers, Gotha;
M. Hermann Goldschmidt, Paris;
Dr. Forster, Berlin; and
Lieutenant Safford, Chicago,

were balloted for and duly elected Associates of the Society.

The Semidiameter of the Moon according to M. Hansen's Tables of the Moon, compared with the results of the best Observations. By J. A. C. Oudemans.

(*Translated from a translation into French communicated by M. Hoek.*)

Though complaints are made of the uncertainty of the planetary diameters, as obtained from micrometric measure-

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ments, yet the diameter of the Moon is no better known to us. In 1851, 53, 54, and 58, M. U. De Lange and I have observed many occultations of the stars, which I have used to calculate the longitude of Batavia. The observations indicate clearly that the semidiameter of the Moon, as given by the Tables of M. Hansen, has need of a negative correction. To control this indication, I have brought together all the determinations known to me of the aforesaid element by means of total and annular eclipses, of occultations of stars, and of measurements made by the aid of a heliometer. I remark, first of all, that M. Hansen has adopted the arithmetical mean which the meridian instruments at Greenwich have given for the two diameters of the Moon, horizontal and vertical. Then, whatever errors there were in the expressions according to which Burckhardt has calculated his Tables of Lunar Parallax, these errors have an influence on the semidiameters deduced from observations at different periods. The most part of the calculations that I have employed give the correction of the semidiameter of Burckhardt's Tables. It was necessary then to reduce them to M. Hansen's Tables. I have done this by means of Mr. Adams' Tables (*Nautical Almanac*, 1856). The following are the details of my results :—

A. Occultations of the stars by the Moon.

1. Occultations of the *Pleiades*, 29th Aug. 1820, calculated by Rosenberger (*Königsberger Beobachtungen* IX *Abtheilung*, p. v.).

As the observation of this phenomenon had succeeded very well at Königsberg, and the positions of the stars were exactly known, Rosenberger has deduced from it the correction of the semidiameter of the Moon. Four immersions combined with six emersions give him

Correction of Burckhardt's Semidiameter .. .. = + 0·07

I find :—

Reduction of Burckhardt's Tables to those of Hansen .. = − 0·93

Correction of Hansen's Semidiameter .. .. = − 0·86

2. Occultation of θ' *Tauri*, 28th March, 1830, which M. Kaiser has used for the determination of the longitude of the Observatory of Leyden (*Memoirs of the R. A. S.*, vol. x. p. 303).

The immersions as well as the emersions were observed at Leyden, Dorpat, and Mannheim. The final equation—

$$\text{Corr. of the Semidiameter} = 2''{\cdot}94 - 0{\cdot}0023\,\delta(\beta - b) + 0{\cdot}0004\,\delta\pi,$$

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is perfectly applicable to our purpose. It will be sufficient to consider as zero the two quantities $\delta(\beta - b)$ and $\delta\pi$, which at most are not more than some seconds: whence—

Correction of Burckhardt's Semidiameter	..	+ 2".94

But we have:

Reduction of Burckhardt's Tables to those of Hansen		− 3".43
Correction of Hansen's Semidiameter	.. ..	− 0".49

3. Occultation of *α Tauri*, 10th February, 1832, also employed by M. Kaiser.

The complete observation was successful at Mannheim, Cambridge, Aberdeen, and Greenwich. The equations give:

$$\text{Correction of the Semidiameter} = + 0''.33 - 0.055\,\delta\pi.$$

I find:

$\delta\pi$ = Parallax according to Adams − that according to Burckhardt .. .. .. = − 0".46

Then:

Correction of Burckhardt's Semidiameter	..	..	= + 0".36
Reduction to Hansen's Tables	..	..	− 1".47
Correction of Hansen's Semidiameter	..	..	− 1".11

4. Occultation of the *Pleiades* 10th Aug., 1841, calculated by Lejeune (*Dissertatio Astronomiæ Inauguralis*, Lugd. Batav., 1845).

An excellent determination resting on a great number of observations. Fifty-two equations combined according to the method of least squares, gave: $\delta l = + 1''.33 - 0.259\,\delta\pi$ with a probable error ± 0".09; here $\delta l = \pi\,\delta\varkappa$, where π is the parallax, $\varkappa$ the ratio (0.2725) of the sine of the semidiameter to the sine of the equatorial horizontal parallax. The correction of the semidiameter is therefore—

$$\pi\,\delta\varkappa + \varkappa\,\delta\pi = \delta l + 0.2725\,\delta\pi$$
$$= 1''.33 + 0.0135\,\delta\pi.$$

I find—

$\delta\pi$, or the parallax according to Adams − that according to Burckhardt .. .. = + 3".44

whence it follows,—

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Correction of Burckhardt's Semidiameter	..	+ 1′·83
Reduction to Hansen's Tables ..	..	− 2·49
Correction of Hansen's Semidiameter ..	..	− 1·11

B. Total Eclipses of the Sun.

1. The Eclipse of July 7, 1842. M. Olufsen's calculations (*Astron. Nach.* xxii. p. 217) gave him,—

$$\pi \delta x = -2''{\cdot}05 - 0{\cdot}296\,\delta\pi$$

add to this—

$$x \delta\pi = \quad 0{\cdot}2725\,\delta\pi$$

and we shall have :

Correction of Burckhardt's Semidiameter $= -2''{\cdot}05 - 0{\cdot}0235\,\delta\pi$.

Mr. Adams' Tables (*Nautical Almanac*, 1856) give for this date $\delta\pi = -0''{\cdot}4$, therefore

Correction of Burckhardt's Semidiameter	..	= − 2′·04
Reduction to Hansen's Tables ..	..	= − 1·46
Correction of Hansen's Semidiameter ..	..	= − 3·50

The same eclipse has been calculated by Carlini. Unfortunately I could not at Batavia consult his Memoir, which is in the *Giornale dell' Instituto Lombardo*, vol. iv.

M. Santini has quoted the results in his account of the eclipse of July 28, 1851, these results differ very much from those of M. Olufsen : "Il semidiametro lunare," says Santini, "poi corrispondente alla parallasse equatoriale di 60′ fù dal Signor Carlini assegnato = 16′ 20″·4."

Burckhardt's Tables give for this date a parallax of 59′ 58″·8. The discussion of the observations by Carlini has therefore given a semidiameter during the eclipse of 16′ 20″ 07, while Burckhardt's Tables give 16′ 20″·7.

The result is :

Correction of Burckhardt's Semidiameter	..	− 0′·63
Reduction to Hansen's Tables ..	..	− 1·46
Correction of Hansen's Semidiameter ..	..	− 2·09

2. The total Eclipse of July 28, 1851, of which there are a great number of observations, but which have not yet been completely discussed. Santini submits the observations of ten observatories to calculation, and has deduced from thence the

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corrections of latitude and longitude of the Lunar Tables (*Ast. Nach.* xxxiv. p. 289).

The observations at Dantzig have been employed there in the determination of the longitude of that place.

The four phases observed at Dantzig, when the erroneous quantity in the 12th equation + 5″·36 is replaced by — 5″·36, give :

Correction of the Semidiameter of the Sun	..	— 0′·65
,, ,,	Moon (Burckhardt)	— 0′·75

while the observations at Königsberg where the eclipse was also total, gave to Santini :

Correction of the Semidiameter of the Sun	..	— 3′·07
,, ,,	Moon (Burckhardt)	— 0′·70

The first of these two last quantities attains a value, improbable because of its magnitude, and incompatible with the results of the observations at Dantzig. The second, for this reason, deserves little confidence. But, fortunately, we have the researches of Wichmann, who introduces into his calculations the measurements of the distance of the horns (*Ast. Nach.* xxxiii. p. 309), and who obtains :

Correction of Burckhardt's Semidiameter of the Moon — 0″·79

which agrees well enough with the result of the Dantzig observations.

We have, then, for the date of this eclipse : —

Parallax according to Burckhardt (*Naut. Alm.*)	=	60′ 30″·2
Correction according to Adams (*Naut. Alm.*)	=	—0″·3
Parallax according to Hansen ..	=	60 29·9
Semidiameter corresponding to this Parallax	=	16 30·75
Semidiameter according to Burckhardt (Memoir by Santini)	=	16 29·2
Correction of Burckhardt's Semidiameter ..	=	—0·75
Reduction to Hansen's Tables ..		—1·55
Correction of Hansen's Semidiameter ..	=	—2·30

C. Annular Eclipses of the Sun.

1. The annular Eclipse of the 7th September, 1820, of which the observations have been calculated by several astronomers, Walbeck, Santini, Rümker, Wurm, and Bürg.

Walbeck has determined the moment of the conjunction according to the observations of seven observatories. Those of Göttingen, Cuxhaven, Bremen, and Mannheim, places where

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the eclipse was annular, gave him, for the correction of the semidiameter, according to Burckhardt's Tables, the following quantities:—

The observations of		Gauss	+2".73
,,	,,	Harding	+3".79
,,	,,	Struve	+2".20
,,	,,	Walbeck	+2".02
,,	,,	Olbers	+1".06
,,	,,	Gildemeister	+0".85
,,	,,	Tralles	+0".10
,,	,,	Nicolai	+1".66
,,	,,	Heiligenstein	+1".21
		Mean	+1".74

I have combined the results of Walbeck's calculations in another manner.

The four astronomers whose names occur first in the preceding table, all observed at Göttingen; Olbers and Gildemeister at Bremen; Nicolai and Heiligenstein at Mannheim. Now it is clear that the local irregularities of the limb of the Moon have influenced alike all the observations in one place. I have, therefore, preferred the following combination:—

Mean of the observations at		Göttingen	+2".68
,,	,,	Bremen	+0".95
,,	,,	Cuxhaven	+0".10
,,	,,	Mannheim	+1".44
		Mean	+1".29

The semidiameter of the Moon, having been adopted by Walbeck = 881".02 (*Correspondance Astronomique*, iv. p. 501), we have, according to these calculations:—

Semidiameter of the Moon according to the observations of the Eclipse of the 7th September, 1820 .. .. 882".31

The calculations of Santini are mentioned at the same time as those of Walbeck in the report by Von Zach. "M. Santini," he says, "has also calculated a very great number of observations of this eclipse; he is working at a memoir, which he is going to publish shortly. In the meantime, these are some of the results which he has communicated to us:—

Correction of Burckhardt's Semidiameter of the Moon +1".39

I, therefore, admit here the same value of the semidiameter

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as in Walbeck's calculations. On this supposition Santini's calculations lead to the result:—

Semidiameter of the Moon, 7th September, 1820 = 882″·41

Rümker's calculations (*Berliner Astronomisches Jahrbuch*, 1824, p. 153) embrace the observations made at 18 places. Among them are Amsterdam, Bergen, and Zurich, where the eclipse was annular, and also the four places already mentioned on the occasion of Walbeck's calculations. Rümker adopted—

Semidiameter of the Moon = 881′·0

and he found

Correction .. = +0′·14

True Semidiameter .. = 881′·14

Bürg's results rest on a total of 51 observations made in twenty-one places (*Berliner Astronomisches Jahrbuch*, 1824, p. 119). The elements of his calculations deduced from his own tables assume

Semidiameter of the Moon = 883′·1

a quantity for which the observations indicate

Correction of = − 2′·3

from which it follows that

True Semidiameter = 880′·8

Lastly, the *Berliner Astronomisches Jahrbuch* of 1825, p. 89, contains Wurm's calculations. A total of 172 observations made at 79 places were compiled by this calculator. Although he was obliged to reject many, particularly at the commencement of the eclipse, he succeeded in basing his results upon a number of observations far larger than that employed by the other calculators. He finds—

Correction of the Semidiameter adopted −2′·18

Semidiameter adopted in the calculations 882′·90

True Semidiameter of Sept. 7th, 1820 = 880′·72

By means of Mr. Adams' Tables I find—

The Semidiameter according to Hansen's Tables = 883″·35

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from whence it follows:—

Correction according to	Walbeck	$= -1''.04$
,, ,,	Santini	$= -0''.94$
,, ,,	Rümker	$= -2''.21$
,, ,,	Bürg	$= -2''.55$
,, ,,	Wurm	$= -2''.63$

Of these results the last is most to be depended upon, as it is based upon the greatest number of observations.

2. The annular Eclipse of May 15th, 1836. Rümker's calculations (*Astr. Nachr.* xiv. p. 97) give the expressions for the moment of conjunction, but not the other results.

If we introduce, for the places whose observations embrace the commencement and the end of the eclipse, the condition that these two phenomena ought to give an equal value for the moment of the conjunction, we obtain the relations between the sum of the corrections of the semidiameters of the Sun and Moon $= x$, the difference of these two quantities $= y$, the correction of the latitude of the Moon $= \delta\beta$, and the correction of the parallax of the Moon $= \delta\pi$.

The results of Rümker's memoir give me, according to the method of least squares—

x	$= -1''.18 + 0''.106\,\delta\pi$	$= -0''.98$	Weight	$= 72''.11$
y	$= -0''.69 + 0''.026\,\delta\pi$	$= -0''.64$	,,	$95''.06$
$\delta\beta$	$= -0''.72 + 0''.586\,\delta\pi$	$= +0''.39$	,,	$8''.62$

seeing that we have for the difference of the parallax according to Burckhardt and Adams

$$\delta\pi = +1''.90.$$

But as Rümker had already admitted in his calculations values

of $x = -1''$
,, $y = -2''$
,, $\delta\beta = -7''.63$

we have the following corrections of the data of the *Nautical Almanac* employed by Rümker:—

Total	$x = -1''.98$
Total	$y = -2''.64$
Total	$\delta\beta = -7''.24$

of which the two first give—

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Correction of Burckhardt's Semidiameter of the Moon	+ 0″·33
Reduction to Hansen's Tables	− 1·97
Correction of Hansen's Semidiameter	− 1·64

The observations of this eclipse do not agree with each other; I find the

Probable error of each observation			= 4ˢ·9
Therefore that of x			= 0″·58
,,	y		= 0·50
,,	$\delta\beta$.		= 1·50
,,	$\frac{1}{2}(x-y) = \sqrt{(0·29)^2 + (0·25)^2} = 0·38$		

D. Measurements with a heliometer.

1 and 2. Observations by Bessel of September 2, 1830, and of December 26, 1833, dates of a total Eclipse of the Moon. Each of Bessel's results is the mean of six diameters, taken at intervals of 30° of the angle of position. He finds,

	2 Sept. 1830.	26 Dec. 1833.
Correction of Burckhardt's Semidiameter ..	0″·00	+ 0″·73

But we have

Reduction to Hansen's Tables	− 1″·42	− 1″·78
Reduction to Hansen's Semidiameter ..	− 1·42	− 1·05

As to the difference of these two results, Bessel himself attributed them to the inaccuracy of the tables of the parallax (*Ast. Nach.* xi. p. 410). But M. Hansen's more accurate tables only reduce them one-half.

3. Measurements of Wichmann of July 1, 1846 (*Ast. Nach.* xxix. p. 1). In this series of observations the measurements follow at intervals of 5° of the angle of position. The observations were corrected for the refraction, according to the new formulæ. The phase was taken account of.

The measurements indicate that the Moon is not a sphere. The differences between the measured diameter are as high as 2″·48, a quantity which we cannot attribute to faulty observations. The mean gave,

Correction of Burckhardt's Semidiameter ..	+ 1″·26
Reduction to Hansen's Tables	− 2·66
Correction of Hansen's Semidiameter	− 1·40

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4. Measurements by M. Peters during the total Eclipse of the Moon, of the 6th January, 1852, which were made under such unfavourable circumstances that I thought it better to reject the results. The eclipse began at $19^h\ 22^m$ mean time of Königsberg, while the Moon was at a height of 12° above the horizon, 38 minutes before sunrise. Peters himself said of them, "Der Mond erschien um diese Zeit wegen seines niedrigen Standes hinter Dünsten des Horizonts und wegen der eintretenden Morgendämmerung so schlecht erleuchtet dass ich nur die Schwächste Vergrösserung anwenden könnte. Mit dieser sah ich indess das Nord-und Süd-rand deutlich" (*Ast. Nach.* xxxiv. p. 11). The result was,

Correction of Burckhardt's Semidiameter—

By 5 measurements in the angle of position 0° ..	$= -1''\cdot1$
6 ,, ,, 90°	$= +0''\cdot8$
Mean	$-0''\cdot15$
Reduction to Hansen's Tables	$-2''\cdot09$
Corrections of Hansen's Semidiameter	$-2''\cdot24$

Summary.

Corrections of the semidiameter of the Moon deduced from M. Hansen's Tables:

A. By occultations of Stars:

1.	Occultation of the Pleiades, August 29, 1820 ..	$-0''\cdot86$
2.	,, θ Tauri, March 28, 1830 ..	$-0''\cdot49$
3.	,, α Tauri, February 10, 1832 ..	$-1''\cdot11$
4.	,, the Pleiades, August 10, 1841 ..	$-1''\cdot11$
	Mean	$-0''\cdot89$

B. By total eclipses of the Sun:

1.	Eclipse of July 7, 1842	Olufsen	$-3''\cdot50$	$-2''\cdot80$
		Carlini	$-2''\cdot09$	
2.	,, July 28, 1851			$-2''\cdot30$
	Mean			$-2''\cdot55$

C. By annular eclipses of the Sun:

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1.	Eclipse of September 7, 1820	− 2″·63
2.	,, May 15, 1836	− 1·64
	Mean	− 2·13

D. By measurements with a heliometer:

1.	Measurements by Bessel, September 2, 1830 ..	− 1″·42
2.	,, ,, December 26, 1833 ..	− 1·05
3.	,, Wichmann, July 8, 1846 ..	− 1·40
	Mean	− 1·29

As to these results, it is evident that the total eclipses of the Sun point to a diameter smaller than that which the other phenomena explain. In fact, the observations here give us the disappearance and reappearance of the light of the Sun in the gaps presented by the indentations of the limb of the Moon. In the annular eclipses, on the contrary, we should find the diameter at its maximum, if the observers noted the moment when the limb of the Sun showed without any interception by the mountains of the Moon. But there are other observers who note the first and the last instant of the appearance and of the disappearance of the series of luminous points, and it is clear that their observations will give a minimum diameter. It is to this difference of conception that Nicolai already attributed the sensible difference between the observations of the same eclipse and at the same place, but by different persons. It therefore appears to me better to separate in the final result the date furnished by eclipses, from those based upon occultations and direct measurements.

I obtain:

Correction of the Semidiameter in M. Hansen's Lunar Tables—

For the calculations of occultations	− 1″·09
,, ,, eclipses	− 2·34

Without doubt there are many memoirs on the determination of longitude by means of the occultations of stars which would furnish us with ulterior data for the subject on which we are engaged. It is true that there are a great number of calculations which do not give the necessary differential equations. Wurm and Triesnecker have probably not even calculated them. But there is in the *Ast. Nach.* a very great number of calculations of longitudes. The index of the volumes i. to xx. has 14 pages, each of 2 columns, of them; that of vols. xxi. to xl. 3 pages. Besides there are 3600 observations of occultations which we find in the same journal, vol. i. to xlviii.

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Lastly, we must not forget the millions of analogous observations communicated in the *Astronomisches Jahrbuch* (1776–1830), the *Allgemeine Geographische Ephemeriden* (1798–1800), the *Monatliche Correspondenz* (1800–1813), the *Zeitschrift für Astronomie* (1816–1818), the *Correspondance Astronomique* (1818–1826),—materials sufficient for an extended research, which my occupations do not permit me even to attempt. I shall content myself with having obtained a result which is sufficient for practical purposes. For we must not forget that the Moon is not a spherical body, and that the most perfect acquaintance with its mean diameter will not enable us to avoid small differences in the result of occultations.

For practice, then, I prefer to admit the following data:—

Relation of the diameter of the Moon to that of the Earth's equator.

According to Hansen	.. .. ..	0·272957
Correction corresponding to	$\Delta r = -1''\cdot09$ ..	−318
	. . $-2\cdot34$ ..	−684
Corrected relation ..	.. .. ..	0·27264
	.. .. ..	0·27227

Values of which that adopted by Burckhardt, 0·2725, is about the mean.

Finally, as the mean equatoreal horizontal parallax is 56′ 59″·57 according to M. Hansen, we have —

Semidiameter of the Moon:

	′	″
According to occultations and direct measurements by the heliometer	15	32·27
According to eclipses of the Sun	15	31·02

Batavia, April 20, 1859.

Results of some Observations on the Bright Granules of the Solar Surface, with Remarks on the Nature of these Bodies. By William Huggins, Esq., F.R.S.

I employ the word *granules* in preference to the other names* which have been proposed for the bright particles of

* Stone calls them *rice-grains;* Nasmyth, *willow-leaves;* Dawes, *granulations* and *minute fragments of porcelain;* Chacornac, *crystals;* Brodie, *shingle-beach.* Sir W. Herschel's *corrugations* and *bright nodules* may refer to the groups of granules and possibly sometimes to single granules.

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the solar surface, because, as Mr. Dawes, who suggested this term, well observes "the appellation *granulations* or *granules* assumes nothing either as to exact form or precise character."

In this paper I confine my remarks to the bright granules as they appear on those parts of the Sun which are free from the disturbing forces or currents which are active in the areas of the spots. Under the influence of the forces by which the spots are produced the bright granules assume different, and with respect to their appearance on the general surface of the Sun, irregular and unusual forms. In these regions of disturbance the granules often appear to coalesce, sometimes to be drawn out into very elongated forms, and occasionally to be wholly dissipated within the umbra of a spot. On those parts of the Sun however where the spots are absent these granules appear to preserve, within not very wide limits, considerable general definiteness of form, of size, and of mode of grouping. It is to these normal characters only of the bright granules that the observations of the present paper refer.

In a note I give references to the more important observations of others of these interesting bodies.*

Distribution.—With the exception, mentioned already, of the areas containing spots, the bright granules are to be seen over the whole surface of the Sun. Occasionally, granules preserving their normal characters may be detected in the penumbræ and the umbræ of spots. On one occasion, May 4, 1866, I resolved a large facula near the edge of the Sun's disk into an aggregation of similar particles. Without the border of the facula were two or three isolated granules of equal brightness with those composing the facula. These granules appeared as if they had become detached from the group forming the facula.

Form.—When the granules are observed with powers of about 100 diameters, no comparison which has been made appears to me so appropriate as that to "rice-grains," suggested by Mr. Stone. If, however, higher powers are employed, this apparent regularity of figure and of size of the granules disappears to a great extent. Many of them are then seen to be nearly round, and not of the elongated form of rice-grains. Besides the oval and nearly round granules, may be observed irregular-shaped masses of almost every form. An important character common to all these bodies, whatever their

* Dawes, *Monthly Notices*, vol. xxiv. p. 140 and p. 161; Nasmyth, ibid. vol. xxiv. p. 66; Stone, ibid. vol. xxiv. p. 124; Lockyer, ibid. vol. xxv. p. 237; Brodie, ibid. vol. xxv. 233; Fletcher, ibid. vol. xxv. p. 231; Huggins, vol. xxiv. p. 141; De La Rue, *Memoirs Royal Ast. Soc.* vol. xxxiii.; Chacornac, *Reader*, Jan. 7, 1865. See also for the interpretation of these bodies, Faye, *Comptes Rendus*, tom. lx. p. 138 and p. 468; Brayley, Knight's *Companion to the Almanac*, 1864, 1865, 1866, and *Ast. Register*, vol. ii. p. 75; De La Rue, Stewart, and Loewy, *Researches on Solar Physics*, First Series; Sir J. Herschel, *Monthly Notices*, vol. xxv. p. 152.

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form, is the irregular broken outline by which they are bounded. If, however, these smaller irregularities of figure be disregarded the granules may be described generally as possessing a more or less oval form. The granules appear to me not to be flat disks, but bodies of considerable thickness.

In the interpretation of these bodies it must not be forgotten that minute irregularities which appear almost insignificant in our telescopes would not be little to an observer on the Sun. If the granules could be viewed from a short distance they would appear probably as wildly rugged in irregularity of outline as are the clouds of our sky.

Size.—On April 26, the Sun's image was allowed to pass before the wires of the micrometer placed at a small distance apart. The interval separating the wires appeared to include sometimes two and sometimes three of the granules which were in contact with each other. I found the value of the interval between the wires to be 2″·59. The average size therefore of those bodies may be taken roughly at 1″ in diameter, and the average longer diameter of the more oval particles at about 1″·5. This estimation agrees closely with the size assigned to them by Mr. Stone.

On some small areas of the solar surface the groups appeared to be composed of granules of nearly the same size, whilst on neighbouring areas a considerable difference in size existed between the adjacent granules. Occasionally a much larger granule was seen which might measure from 2″ to 3″ in diameter. Many of the granules were smaller than 1″ in diameter.

Relative position.—On many parts of the Sun the granules, though they lie near together, are detached bodies separated from each other by small intervals. These groups of isolated granules are mingled with tesselated surfaces of bright matter formed of close aggregations of granules. The forms assumed by the groups of closely united granules are very various. Often they appear as nearly round or oval cloud-like masses, and when in this form have been probably mistaken by some observers for single granules. Sometimes these groups are long, irregularly formed bands, suggesting to an observer the rugged, broken sides of a range of mountains. On April 26, nearly in the centre of the Sun's disk, I observed a long oval border of tesselated bright matter, enclosing an area over which the granules were sparsely distributed. Some of the more characteristic of the modes of grouping of the bright granules, which I have observed on different occasions, are given in a diagram which accompanies this paper.

It is in connexion with these groups that the coarse mottling of the solar surface originates, for the difference in brightness of adjoining areas is produced *mainly* by the greater or less degree of closeness of aggregation of the bright granules. A second cause which contributes to the formation of the mottled appearance of the Sun's disk is to be found in the

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different degrees of brightness of the material which fills the intervals between the groups of granules, and between single granules.

In addition to these phenomena, a careful observer will notice appearances which suggest considerable inequalities of *level* in the bright surface of the Sun. The whole photosphere appears corrugated into irregular ridges and vales. Over this uneven surface, not unlike that of a stormy sea, the groups

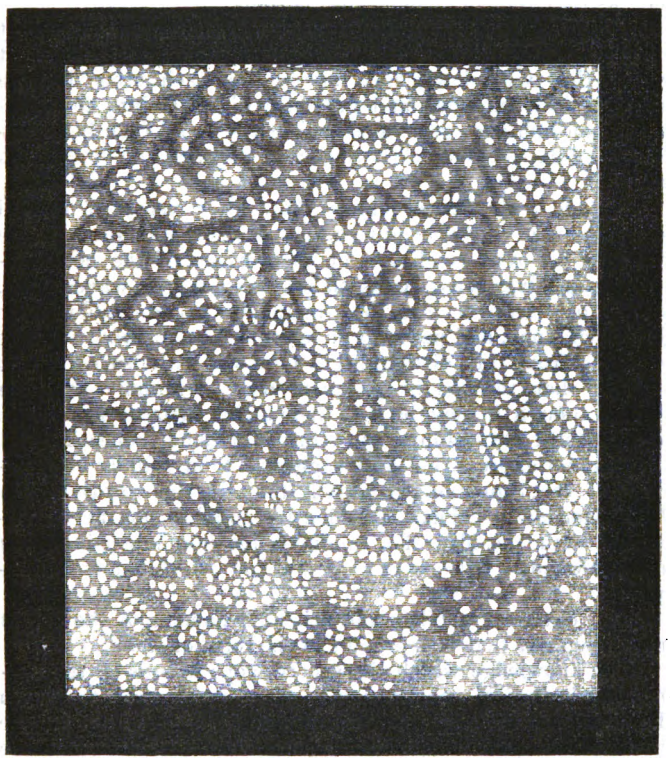

Diagram of the Distribution of the Bright Granules on the parts of the Sun which are free from Spots.

of granules which have been described are irregularly distributed.

Superposition?—I have not been able to satisfy myself whether the material of the photosphere immediately beneath the bright granules consists of an aggregation of separate particles. The appearances presented suggest that this matter is lower (nearer the Sun's centre), and that generally the granules are

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bodies of considerable thickness, elevated above it, and surrounded by gaseous matter which is non-luminous in comparison with the extreme splendour of the granules. So superior in brightness are the granules, that the exterior layer, which is composed exclusively of them, must be regarded as the source of nearly the whole of the light, and probably also of the heat, which the Sun emits. Except in the penumbra of a spot, under the influence of unusual currents, I incline to the opinion that the bright granules are not superposed on each other as long as they remain recognisable as such.

The phenomena would be well represented if we might suppose that the granules are recently condensed incandescent clouds, that they slowly sink, merge into each other, become less and less luminous, and gradually dissipate into comparatively non-luminous gas. The dark pores would then be represented by the portions where complete vaporization had taken place.

Nature.—Mr. Dawes states that, after years of careful observation of these bright bodies, he considers them to be "merely different conditions of the surface of the comparatively large luminous clouds themselves—ridges, waves, hills, knolls, or whatever else they might be called—differing in form, brilliancy, and probably in elevation." I would venture to differ from this distinguished observer only so far as to suggest that the bright granules were originally *separate* clouds, though it may be that their under surfaces soon begin to unite with the less luminous stratum of clouds beneath them.

I cannot express the ideas suggested to myself by observations of the Sun more accurately than in the words of Sir John Herschel—"That it is hardly possible not to be impressed with the idea of a luminous medium intermixed but not confounded with a transparent and non-luminous atmosphere."

The extreme mobility and other phenomena of the bright matter suggest that it is present in the form of *cloud*.

The knowledge which the spectroscope affords of the chemical and physical constitution of solar matter, together with the phenomena of terrestrial flame, would suggest that in the greatly different powers of radiation possessed by matter in the solid, liquid, and gaseous state, combined with the processes of condensation and revaporization, a feasible explanation might be found of solar phenomena. However, the law of exchanges, for which we are greatly indebted to the original and important researches of Balfour Stewart, shows that in the case of the Sun, if we suppose it to be, at the least, equally hot throughout its mass, the gas near the surface would not appear dark, as the umbra of a spot or a dark pore does, because its own feeble power of emission would be supplemented by its power in the inverse ratio of transmitting the radiation from the gas, or from the photosphere behind it.

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At present, therefore, we do not appear to be able to explain satisfactorily the small brilliancy of the material between the bright particles, and of the darkness of the pores, which, like the umbra of a spot, appears to be at a lower elevation than the bright matter. It has been recently suggested that the dark umbra of a spot originates in a lower temperature produced by a down rush of cooler gas from above. It is of importance to know whether this hypothesis can be made to afford a probable explanation of the numerous dark pores and the small brilliancy of the material between the groups of bright granules.

If the granules are incandescent clouds, their general oval form may possibly be due to the influence of currents. We have to seek, besides the conditions connected it may be with the high temperature of the Sun, or with some peculiarity when in the gaseous state of the substances present in the photosphere, in consequence of which there exists a general approximate uniformity of size, of form, and of mode of grouping of these incandescent bodies.

Description of an Equatoreal Clock. By Lord Oxmantown.

The following description of an apparatus for giving an equable motion to a heavy Equatoreal of 18 inches aperture, may, perhaps, be interesting to some of the Fellows.

It was constructed last autumn, and its merit, if it has any, is that it can be very easily made. No nice workmanship is required; a joiner and plumber can execute the work with sufficient accuracy.

The motive power is a piece of wood closely fitting a wooden box containing water, on the surface of which this piece of wood floats. This float is covered with canvass saturated with pitch, to prevent it from imbibing water, and so expanding and sticking fast in the box.

It has also pieces of sheet-brass fastened on the edges over the pitched canvass, to prevent the pitch adhering to the sheet-lead with which the box is lined. A tube from the bottom of the box, terminating in a piece of flexible pipe, the extremity of which is kept at a uniform depth below the surface of the water in the box by being suspended from the float, allows the water to escape at a constant rate which is regulated by a valve in this pipe. (*h h*) is the box; (*i i*) the float, which is attached to a brass tube (*d d*), which acts as a guide by passing through a hole in the cross-piece of wood (*f f*), supported by two upright pieces (*g g*); (*n n*) is the India-rubber tube, through which the water escapes; (*o*) the valve to regulate the flow; (*m*) is a cock, which is closed when the clock movement is not required; (*q q*) a second box to receive the water as it flows out of the first. When we wish to employ the clock, we, first, pump up water from (*q*) to (*h*)

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through (p) and (l); secondly, we get the object to be observed into the centre of the field; thirdly, open the cock (m) and clamp the sector (t) to the polar axis (z) by means of the screw (u); and if the object is not now sufficiently in the centre of the field, it may be brought there accurately by turning the nut (c), which draws the sector (t) further from or nearer to the float, by bending the spring (w).

The accuracy of this clock depends, to a great extent, upon the care which is taken to make the sides of the box parallel and flat, so that the horizontal section may be the same at all levels, and to fit the float to it carefully, so that the interval between the float and the sides of the box may be as small as is consistent with absence of friction. Let A be equal to the area in square inches of horizontal surface of float, and $\frac{A}{n}$ area of interval between the float and sides of the box; then, if the float be displaced $\frac{1}{m}$th of an inch from the position of equilibrium, the surface of the water round the float will be displaced through $\frac{n}{m}$ inches; therefore, if w = weight in lbs. of 1 cubic inch of water, the force tending to bring the float back to its former position $= w \cdot A\left(\frac{n}{m}+\frac{1}{m}\right)$; whereas, if the float had rested on a surface of water of infinite extent, the corresponding force would have been $w \cdot A \times \frac{1}{m}$ $\therefore$ the ratio of the forces in the two cases $= n+1 : 1$. In the case of the instrument to which this apparatus has been applied, $A = 1296$, and if, for example, the interval between the float and sides of the box $= \frac{1}{16}$ of an inch, and if the variation of friction of the polar axis be taken at 3 lbs. (the average friction being about 7 lbs.), the displacement of the float, which would have been equal to ·064 of an inch on a surface of water of infinite extent, will become equal to

$$\cdot 064 \times \frac{1}{1+\frac{36^2}{4\times 36\times\frac{1}{16}}} = \frac{\cdot 064}{145} = \cdot 00044 \text{ inches.}$$

As the float is attached to a wire rope the other extremity of which wraps round the sector (t) of 28 inches radius, this deviation corresponds to $7'\ 51''$ of space at the equator in the first case, but only $3''\cdot 2$ in the second case.

Assuming that the flow of the water from the box will be uniform if the head be uniform, the only remaining cause of irregularity lies in this, that the overflow is kept at a constant depth below any fixed point in the float; that point, through variation of the resistance, not being at an absolutely invariable distance below the surface of the water in the box; consequently, the head will vary if the immersion of the float varies.

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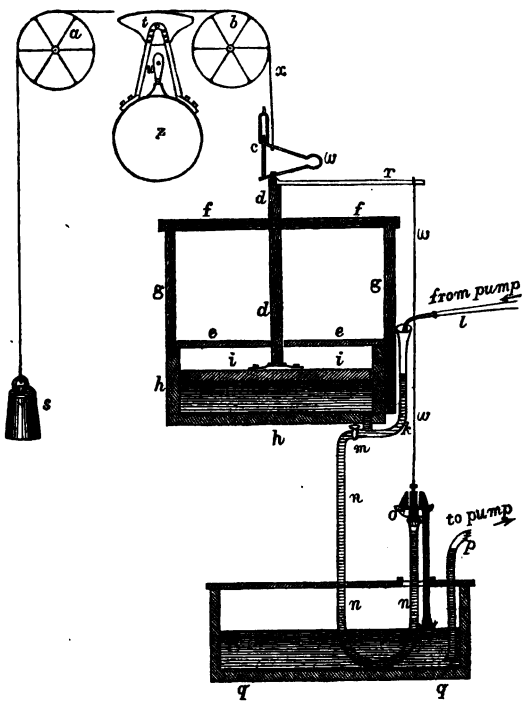
We have seen above that, with a variation of friction of 3 lbs., the variation of immersion = ·064 of an inch; therefore (47 inches being the distance of overflow below the surface of water) the ratio of maximum velocity to mean $= \sqrt{\frac{47 \cdot 064}{47}} = \frac{1 \cdot 00067}{1}$, which corresponds to an angular deviation in right ascension of 36″ of space at the equator per hour.

In practice, however, when the instrument is accurately counterpoised and carefully oiled, the variation of the force required to turn it round in right ascension is probably very much less than this; and if between one night and the next the friction increases through viscosity of the oil, the rate is easily corrected by means of the valve (*o*).

This clock has been used for micrometrical observations, and answers perfectly; but how far for spectroscope purposes it will bear comparison with other contrivances, has not yet been ascertained.

The accompanying figure has been drawn on a scale of $\frac{1}{3}$ inch to 1 foot, with the exception of the sector (*t*) and the weight (*s*), which keeps the rope (*x*) extended; these two parts having been drawn on a reduced scale, and in a different position to save space.



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On the Satellite of Sirius. By M. Otto Struve.

Two years ago I had the honour to communicate to the Royal Astronomical Society the first results of my micrometrical measures of the satellite of *Sirius*, discovered by Mr. Alvan Clark. Though at that time the comparison of the observations made in 1864 with those of the preceding year strongly favoured the supposition that the small star did not participate in the large proper motion of *Sirius*, and, consequently, was not physically connected with it, I indicated the reasons which induced me to discuss these appearances, and suspended therefore the judgment about that conviction at least until the next year, when the differences between the orbital motion and the relative motion in the supposition of a merely optical juxtaposition would have increased to nearly the double amount. At present, after having two years' observations more, we are enabled to submit the question to a stronger examination.

The following list contains all measures that I have proceeded until now to make on the satellite, after having excluded those days on which the state of the atmosphere was indicated as so unfavourable that it did not admit an accurate measure of the distance:—

Date.	Sid. Time.	Power.	Distances.	Position Angle.	
				Observed.	Corr.
	h m		″	°	°
1863·21	7 30	309	10·30	79·8	82·0
63·21	7 11	309	9·99	81·2	83·0
64·18	6 37	309	11·22	75·2	76·7
64·24	7 11	309	10·80	75·6	77·4
64·24	7 33	412	10·73	74·3	75·3
65·17	5 45	(300)	10·39	76·0	77·3
65·24	8 20	(300)	10·80	73·8	77·0
66·19	5 55	309	11·20	73·3	74·7
66·20	6 56	309	10·90	74·4	76·1
66·23	7 4	309	10·70	73·5	74·7

The column headed "Corr." gives the position angles corrected for the systematical errors, as deduced from my observations of artificial double-stars. It might be remarked on this occasion, that by later researches these corrections are proved to have full strength, as well for natural double-stars in all zenith distances, as for artificial ones near the horizon. Concerning my observations, I have further to say that those of the spring, 1865, have been made at Rome, where Father Secchi had kindly offered me for that purpose the use of the excellent 9-inch refractor of the Observatory of the Collegio

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Romano. For the correction of these Roman position angles, I have employed estimated values of the magnifying powers. A small error in this supposition would be of no consequence.

The arithmetical means of each year's observations are—

1863·21	$\rho = 10''\cdot15$	$P = 82^\circ\cdot50$	2 days.
64·22	10·92	76·50	3 ,,
65·20	10·60	77·15	2 ,,
66·21	10·93	75·15	3 ,,

Adopting now the same weight for these four results (a supposition that appears necessary if we admit for different years different constant errors of estimation with regard to the centre of the bright star), the method of least squares conducts us to the following formulæ for a uniform rectilinear relative motion in the course of three years:—

$$p = +(10''\cdot186 \mp 0''\cdot193) + (0''\cdot121 \mp 0''\cdot046)\ (t - 1863\cdot0)$$
$$q = +(1''\cdot523 \mp 0''\cdot193) + (0''\cdot429 \mp 0''\cdot046)\ (t - 1863\cdot0)$$

p and q being the rectilinear co-ordinates of the satellite from the principal star. From these formulæ we deduced the observed change for three years; in distance $= +0''\cdot63$; in position angle $= -6^\circ\cdot38$.

If the small star formed only with the large one an optical double star, the mean proper motion of *Sirius* would have changed in three years the relative distance by $+2''\cdot56$, and the angle of position by $-15^\circ\cdot02$,—values that might be increased to $+2''\cdot81$ and $-16^\circ\cdot17$, if we introduce the ascertained inequalities of that proper motion. Evidently our observations do not agree with similar changes; therefore this hypothesis must henceforth be given up.

Moreover, the excellent investigations of Dr. Auwers (*Ast. Nach.*, No. 1506), on the orbital motion of *Sirius*, demands for the disturbing body in the indicated interval an increase of the distance by $0''\cdot55$, and a diminution of the position angle by $5^\circ\cdot31$; values agreeing so nearly with those deduced from our micrometrical measures, that we scarcely can doubt that the satellite discovered by Mr. Alvan Clark is really the disturbing body.

The distance itself cannot form in this case a criterion for the identity, for the irregularities of the proper motion could only furnish the distances of the common centre of gravity. The distances of the disturbing body, as given by Dr. Auwers, must therefore have been derived from a supposition of the relative masses of the two bodies. Putting $d = 3\cdot05\ \Delta$ (Δ being the distance of the centre of gravity), Dr. Auwers, it seems, has adapted the supposition on the masses to the distances of the satellite, as observed before his Note of 1864 was

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drawn up. Our measures agree so nearly with the supposition that the mentioned relation would only be changed into $d = 3{\cdot}087\ \Delta$. Of course, even a considerably more important change in this supposed relation, would affect the change of the distances, during the short interval of our observations, only by very small quantities.

With regard to the position angles, on the contrary, not only their changes, but also their values themselves, as measured on the satellite, must agree with those deduced for the centre of gravity of the system, if really that satellite is the body that has produced the irregularities of the proper motion. Here we have the following comparison for our yearly measures:—

1863·21	Str.—Auw.	$= -0^{\circ}{\cdot}96$
64·22		$-5{\cdot}11$
65·20		$-2{\cdot}73$
66·21		$-2{\cdot}93$

This near agreement is certainly one of the strongest indications for the supposed identity. In the mean the difference is $-2^{\circ}{\cdot}94$, which at the mean distance $10''{\cdot}64$ corresponds to $-0''{\cdot}55$ in space. I do not think this difference can be attributed to the accidental errors of my observations, and even the supposition of constant errors of estimation with regard to the centre of the bright star as depending from the sensibility of the eye for the colours of the atmospheric spectrum of *Sirius*, is at least rendered much less probable by the circumstance than my Roman observations, where that star in its higher elevation did not offer any trace of spectrum, very nearly agree in amount and sign with the mean of the other three determinations. Therefore it seems that in this respect Dr. Auwers' theory is still susceptible of a small correction. That such a correction might be desirable will certainly not appear surprising, if we consider the unequal qualities of the numerous observations from which he has derived the disturbances of the proper motion.

Admitting then that the observed satellite is identical with Bessel's obscure body, the above given relation $d = 3{\cdot}087\ \Delta$, indicates, that its mass must be estimated approximately half that of *Sirius* itself. If both bodies had the same physical constitution, this relation of the masses would assign to the globe of the satellite a diameter only 1·26 times smaller than that of the principal body, and therefore, considering the extraordinary brightness of the large star, we would be induced to place also the satellite in the first class of magnitude. With this conclusion the observed brightness of the companion forms a manifest contradiction. It is commonly said to be of the 9th or 10th magnitude; and only in the Spring of 1864 I have noted it once as of the 8th magnitude, probably on account of

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an extraordinary favourable state of the atmosphere. Hence it follows that, to maintain the identity, we must admit that both bodies are of a very different physical constitution. That the light of the satellite is gradually increasing, as I was inclined to suppose from the comparison of my observations of 1864, with those of 1863, has not been confirmed by later observations; but in our latitude the estimation of the brightness depends too much from the condition of the atmosphere to admit of an accurate judgment in this respect.

Pulkova, April 15, 1866.

Supposed Observation of Biela's Comet.

By James Buckingham, Esq.

At our evening Meeting in March, an inquiry was made "if Biela's Comet had been seen;" having mentioned an observation I obtained on November 9 last of two bodies which were supposed by me to be those of that Comet, our President requested that "details should be furnished to the Society." But business affairs caused me to leave London the following day, and I have only returned a few days since, and now beg to communicate the observation, viz.—

Comets + Star.

Difference of R.A.

Nov. 9, 1865, at 9 h 32 m G.M.T. A + 28 s and was some minutes N. of B.

B + 37·75 same decl. as ✳ comparison.

	h	m	s	°	′	
✳ *a* is 12th or 14th mag.	23	18	35	R.A. 77	25·1	N.P.D.
Deduced place of A	23	19	3			
B	23	19	12·75	77	25	

Owing to clouds, a second position could not be obtained.

It may be necessary to explain why no better observations could be secured, having usually at hand well-appointed means. But having been many weeks away in Scotland, returning only in the beginning of November, I was favoured with Mr. Hind's Ephemeris of Biela, and sought several nights with my 9-in. Equatoreal (by Wray, and which is a very fine one) unsuccessfully, I therefore put up my 20-in. object-glass (the instrument having been out of use for several months, for some alterations of the mounting, and was only approximately in the meridian), after searching near the predicted places, in sweeping found two round vapory bodies near each other; after watching several minutes motion was detected (from n.f. to s.p.) of the small one, which appeared most condensed, but without any

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sign of nucleus, but yet with nearly defined outline. I had no micrometer, but only a pair of thick cross-wires in the eye-piece (negative), which are seen clearly against the sky; and observing a small star "*a*," apparently on the same parallel as the brightest object; got some transits and approximate positions without being able to determine their accurate ones, but the star *a* was nearly north of 70 *Pegasi*; therefore went to my 9-in. Equatoreal, but could not see any trace of the new objects. On returning again to the large (20″) instrument, again saw both objects, and the one B I thought had moved yet further and past a very minute star, in the same direction as before; but clouds came on, and no opportunity occurred, as on 12th November I left London, and did not return till the 23rd, when I again searched, but could not find either of the new objects, but found the star of comparison and determined its place, and having made a rough ephemeris, by using the observed differences on November 9 and Mr. Hind's calculated places, swept for several weeks at every opportunity. I need perhaps scarcely say "my conviction is, the bodies observed were comets;" and from the circumstance of being near each other, although closer than calculated, it is presumable, as so near its orbit, they were Biela's.

A gentleman at the Meeting in March remarked he "had a doubt if motion could be detected in half an hour." There could not be the slightest doubt of the fact, and taking Mr. Hind's ephemeris as very good data, it appears the absolute arc in that time would be about 26 or 27 seconds; a distance impossible under like circumstances to be overlooked.

I must apologise for the length of this letter, but must mention I had the honour of an interview with Mr. Hind on the morning of November 10, who patiently heard my description, was much interested, but had a very strong impression "the bodies observed could not be Biela, in consequence of the places not agreeing with the calculated ones, and so close together, but that they were nebulæ;" and therefore no communication was made to the Society, as I hoped to obtain future positions, but was prevented, as described, by leaving town.

Westmorland House, Walworth Common,
London, S., May 4, 1866.

Note on Kirch's Variable in Collo Cygni.

By E. J. Stone, Esq.

In 1686 Geo. Kirch discovered that a star in *Collo Cygni* was subject to considerable variations of magnitude. This star was considered by Kirch to be identical with the 5th-magnitude star χ *Cygni* of Bayer. In Flamsteed's *Historia Cœlestis Britannica* will be found observations of a 5th-mag-

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nitude star in *Cygnus*, which is there called χ *Cygni*. This star is not the variable. Flamsteed's star will be found in Bessel's *Fundamenta*, No. 2517, and upon it Bessel remarks, "Flamsteedius stellam per χ designat, sed stella a Bayero ita dicta alia est neque reperitur in Catalogo." The neighbouring 5th-mag. star is contained in *Piazzi*. Piazzi has followed Flamsteed's nomenclature. The star in Piazzi xix. 295, *Cygni Variabilis*, 6.7 mag., would appear to be the star *e* of Kirch's map. It certainly is not Kirch's variable. In the notes to Baily's *Flamsteed* will be found the following, "17 *Cygni* χ. M. Bessel does not think that this is the star designated by the letter χ in Bayer's map. But there is no other star of that magnitude (5th) observed by Flamsteed or Piazzi that comes nearer to the position there indicated. I apprehend that the error is in Bayer, as there is no star in the Catalogue either of Ptolemy or Tycho that will accord with the position he has given." Baily has been followed in these remarks by Admiral Smyth, for in *Celestial Cycle*, page 460, vol. ii. we find, "This extraordinary star has been mistaken for χ, a 5th-magnitude star, which precedes." A careful comparison of Bayer's map with that given by Kirch, *Philosophical Transactions*, No. 343, 1715, will, I think, most clearly show that the variable is really the χ *Cygni* of Bayer. It would appear that the variable must have been near its maximum at the time Bayer was engaged on this part of the heavens, and that the variable was then brighter than the neighbouring 5th-magnitude star of Flamsteed, to which no letter has been appropriated by Bayer. This circumstance is to be regretted. Flamsteed's star is contained in Piazzi and many of our modern standard Catalogues, and is in them called χ *Cygni*. From our having two stars both called χ *Cygni* many mistakes have arisen. Mr. Pogson, in his lists of Variables, *Radcliffe Observations*, 1854, and *Monthly Notices*, 1856, Dec. 12, has given the R.A. and N.P.D. of Flamsteed's χ *Cygni*, instead of those of the variable. In subsequent lists Mr. Pogson has corrected this error. But the same mistake will be found in all the editions of Sir J. Herschel's *Outlines*, Chambers' *Hand-book*, and probably in other works. I am of opinion that some steps should be taken to at least palliate the confusion at present existing from our having two stars called χ *Cygni*. No method entirely without objection can probably be proposed. I think, however, that the least objectionable method would be to call the star contained in Flamsteed, Bradley, Piazzi, and our chief modern Catalogues, χ_1 *Cygni*, and then to call the variable χ_2 *Cygni*. We should thus have for

1866, Jan. 1.

		R.A.	N.P.D.
		h m s	° ′ ″
χ_1 Cygni	5 mag.	19 41 20·38	56 34 56·67
χ_2 Cygni	variable	19 45 25 ±	57 26 ±

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Spectrum of α Orionis. By the Rev. Father Secchi.

The rather too short letter which I wrote, accompanying the drawing of the spectrum of *α Orionis*, has given occasion to some remarks and inquiries of Mr. Huggins, which require some explanation on my side.

First, I must say that there is no slit in my apparatus, as for the stars it is quite useless and troublesome: the position of the lines is determined and measured as with the double stars. The *original* figure which I sent is made on the scale of one centimeter to one revolution of the micrometrical screw of the eye-piece. It would have been very easy to form a table of such values, but every one can make it, applying a metrical rule to the original figure. All the large lines have been put down by micrometrical measures: the smaller have been delineated by separating the wires only for one revolution, and putting them at the due place. The scale being large, no chance of error exists.

What Mr. Huggins says, that the colours in my figure are not right, is true; but this is an effect of artificial light which changes the tones of the colours; they ought to be omitted altogether. To fix the lines D and C, I have made use of a spectroscope with slit, and lights of sodium and magnesium; therefore they are correct.

The chief point of difference in my spectrum and that of Mr. Huggins lies in the band indicated with the letter *s* in my figure, which is placed by me nearer to ζ than to δ, while in Mr. Huggins' it is quite the opposite (ζ is 1151 of H). This difference is so great that no micrometrical measure could be necessary to acknowledge it.

Not trusting to myself only, I have asked several other persons to try the position, and they agreed with me. I have therefore nothing to correct in my drawing, which was made before seeing those of Mr. Huggins', and carefully verified several times afterwards.

I do not say that there is a mistake of Mr. Huggins: on the contrary, I beg him to verify the measures. As he says that the nebulous band ending with the strong line 1069·5, then he gives perfect reason to me; since this is exactly the band which is controverted, and which I have not seen; but, instead of it, I have seen one in the place nearly 1110 of his scale.

We are, therefore, agreeing perfectly in the deficiency of the band ending 1069·5, but he must inquire if this is not appeared in the place 1110 of his scale.

I did not intend to intrude into the discovery of Mr. Huggins in the lines of *Sirius* and *Rigel*, but only to confirm his discovery. My observation is rather more detailed, since, for the intervals of the lines of *Sirius* and *Rigel*, I have been able to fix the proportion of $6\frac{1}{2}$ to $4\frac{1}{2}$. As to the similarity of these

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spectra with those of Plücker, I have nothing to retract; the number of lines between the red and blue of the ring in *Sirius* and in *Sulphur* is most nearly the same.

Rome, April 16, 1866.

On a New Star. By W. Huggins, Esq.

On May 16 I received a note from Mr. John Birmingham, of Tuam, in which he informs me that, on the night of May 12th, he saw a new star near ε *Coronæ* B. He describes the star as "very brilliant, of about the 2d mag." The same day a letter arrived from Mr. Baxendell; he observed the star on the 15th, and found it to be fully equal in brightness to β *Serpentis* or γ *Herculis*.

On the night of the 16th, I and Dr. W. A. Miller examined the spectrum of this strange star. The results of our observation were communicated on the 17th to the Royal Society.

On the 16th, the star was brighter than ε *Coronæ* B, fully ·5 mag., perhaps ·75 mag. brighter than ε. On that evening a very faint nebulosity was seen extending some little distance round the star, and gradually fading away at its outer boundary. A comparative examination of neighbouring stars showed that this appearance of nebulosity was due to the star itself. On the evenings of the 17th, 18th, 19th, and 21st, no certain indications of a nebulous light about the star could be detected.

The spectrum of this star is very remarkable, and leads to unexpected conclusions as to its physical condition.

The light of the star is compound, and has emanated from two different sources. Each light forms its own spectrum. The principal spectrum is analogous to that of the Sun. The portion of the star's light represented by this spectrum was emitted by an incandescent solid or liquid photosphere, and suffered partial absorption by passing through an atmosphere of vapours existing at a temperature lower than that of the photosphere.

This absorption-spectrum contains two strong lines, a little more refrangible than C of the solar spectrum; a shaded group of lines extending nearly to D; a faint line coincident with D; numerous fine lines up to about the position of *b* of the solar spectrum, where a series of groups of strong lines commences and extends as far as the spectrum can be traced.

The second spectrum, which in the instrument appears to be superposed upon the one already described, consists of five *bright lines*. This order of spectrum shows that the light by which it was formed was emitted by matter in the state of *gas*.

One of these bright lines is in the red at the position of

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Fraunhofer's C. The brightest of the lines coincides with F; a little beyond this is a fainter line; near this line a fourth occurs, which is either double or undefined at the edges. In the more refrangible part of the spectrum, probably not far from G, a fifth bright line was seen by glimpses.

On the 17th I observed this spectrum of the star simultaneously with the bright lines of hydrogen produced by the induction spark. The brightest line coincided with the centre of the expanded green line of hydrogen. Apparently the red line also coincided with that of hydrogen, but the faintness of the star spectrum did not permit the coincidence to be observed with certainty.

These bright lines are all much more brilliant than the corresponding refrangibilities of the continuous spectrum over which they fall. We must, therefore, conclude that the temperature of the gas by which the light, consisting of these five refrangibilities only, was emitted, must be much higher than that of the stellar photosphere from which the principal part of the star's light has emanated.

On the 17th, 19th, and 21st, the spectrum of the star was again examined. No important changes had occurred. In the faint spectrum of the fading star, on the 21st, both spectra could be seen. Some additional groups of absorption-lines are probably present in the continuous spectrum, but the gaseous spectrum is not changed otherwise than in the diminution of its brilliancy. The sudden blazing forth of this star, and then the rapid fading away of its light, suggest the rather bold speculation that, in consequence of some great internal convulsion, a large volume of hydrogen and other gases were evolved from it. The hydrogen, by its combination with some other element (the other bright lines do not coincide with those of oxygen), giving out the light represented by the bright lines, and at the same time heating to the point of vivid incandescence the solid matter of the photosphere.

The grouping of the dark lines of the absorption-spectrum is similar to that which characterises the spectra of *α Orionis* and *β Pegasi*, stars in the atmosphere of which *no hydrogen exists*.

We have found that several of the more remarkable of the variable stars which have an orange tint have spectra similar to those of *α Orionis*. It is worthy of notice that all the white and bluish-white stars have spectra in which, on the other hand, the dark lines due to absorption by hydrogen are very strong, whilst all the other lines are thin and faint. These and other observations we have made suggest that hydrogen probably plays an important part in the changes and physical differences of constitution of the stars.

The grouping of the lines of absorption of this new star shows that its colour would be orange, if it were not for the greenish-blue light of the bright lines, which more than

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makes up for the refrangibilities which have been intercepted by absorbent vapours. Mr. Hind writes, "On the 19th, the star was perfectly white and stellar to my eye." Mr. Baxendell's remarkable powers of observation enabled him to divine some of the results of prismatic examination which at that time were not known to him. He writes, "On the 18th I several times received the impression of a bluish tinge as if the *yellow of the star were seen through an overlying film of a blue tint.*"

On the 17th, the star's exact position was obtained at Greenwich. The Astronomer Royal informs me that a meridian observation showed that this strange object "agrees precisely with Argelander No. 2765 of 'Bonner Sternverzeichniss,' declination + 26°, magnitude 9·5." Mr. Baxendell writes to me, "It is probable that this star will turn out to be a variable of long irregular period, and it may conveniently be at once designated T *Coronæ*." Mr. Baxendell gives, in his letter, the following table of the magnitudes of this wonderful star:—

"A reduction of all my observations gives the following results:—

1866		h	m			Mag.
May 15	at	12	0	G.M.T.	T Coronæ	= 3·6 or 3·7
16	,,	10	30	,,	,,	= 4·2
17	,,	11	0	,,	,,	= 4·9
18	,,	12	30	,,	,,	= 5·3
19	,,	12	15	,,	,,	= 5·7
20	,,	12	30	,,	,,	= 6·2."

A diagram of the spectrum, taken from the *Proceedings* of the Royal Society, will, by the permission of that Society, appear in the next number of the *Monthly Notices*.

Prof. Gould, in a letter dated Cambridge, U.S., April 20, 1866, addressed to the President, calls attention to a paper by Mr. Ferrel in the year 1853, in which it is shown that the action of the Moon on the tidal protuberances produces an appreciable retardation in the velocity of rotation of the Earth.

Mr. Ferrel's paper, published in the *Astronomical Journal*, vol. iii. pp. 138–141 (Dec. 8, 1853), is entitled "On the Effect of the Sun and Moon upon the Rotatory Motion of the Earth." After some preliminary remarks, the author proceeds as follows:—

"Assuming the Earth to be a perfect sphere, the effect of the Moon is to raise the waters of the ocean on the opposite sides, and to cause the mass to assume the form of a prolate spheroid.

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If the Earth remained a perfect sphere, or if the major axis of this spheroid coincided with the line joining the centres of the Earth and Moon, the attraction of the Moon could not affect the Earth's rotatory motion. But in consequence of the resistance which the waters of the ocean suffer in their motions, the major axis of this spheroid forms, with the line joining the centres of the Earth and Moon, an angle of about 30°; and the well-known tangential force relative to the centre of gravity, arising from a difference between the attraction of the Moon upon the different particles of the Earth and the centre of gravity, and also from a difference in the direction of this attraction, is not exactly balanced in its action upon the different sides of the mass, in consequence of two opposite parts, in which this force acts in the same direction, being a little more prominent than the other two on account of the spheroidal form which the mass assumes. Hence there is a small residual force of the order a^2, which, acting continually in the same direction, has a tendency to produce a variation in the value of d A.* This force would act upon the waters of the ocean alone, producing a western current, and would not affect the nucleus at all if it did not offer resistance to their motions; but as soon as this resistance becomes equal to the force, the whole force is spent indirectly upon the nucleus; and as, by the principle of the preservation of areas, the action of the particles upon each other cannot change the value of A, the effect is the same as if it acted directly upon the nucleus."

He then proceeds to a mathematical investigation of the foregoing effect; this is presented in what may be thought a form of unnecessary complexity, in consequence of the assumption of a hypothetical law of density of the solid nucleus:† but the result obtained is

$$nt - u = \frac{39\,\mathrm{L} \cdot h \cdot \sin 2a}{\varrho^3 (36m + 16m')}\, t^2$$

where

$1 + h$ = Semi-axis major of the spheroid, the semi-axis minor being $= 1$.

m = Mean density of the Earth.

m' = Density of nucleus at the surface.

---

* a, the order of the disturbing forces; A, the moment of rotation, or sum of the products of each particle into the area described by its radius vector.—Ed.

† The assumed law is—

$$\text{Density} = m' + c\,(1 - r^{\frac{3}{2}}),$$

r being the distance from the centre (the radius being unity) and c being so determined that the mean density shall be $= m$; or finally

$$\text{Density} = 3m - 2m' - 3\,(m - m')\, r^{\frac{3}{2}}.$$

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L = Moon's attraction at distance unity.

ϱ = Distance of centres of the Earth and Moon.

a = Angle between line forming the centres of the Earth and Moon, and the major axis of the spheroid.

nt = Angular motion of Earth when not acted on by the disturbing force.

u = Actual angular motion.

For converting the foregoing formula into numbers the author writes as follows:—

"Now the attraction of the Earth at the distance of the equatoreal radius, is 32·148 feet; and putting the attraction of the Moon $\frac{1}{75}$ of that of the Earth, we shall have

$$L = \frac{32 \cdot 148}{75 \times 20922000} \qquad (17)$$

in times of the Earth's radius. Assuming that the tide caused by the action of the Moon in the open sea is two feet, and that it happens two hours after the Moon passes the meridian, we shall have $h = 2$, and the angle $a = 30$, and consequently sin $2a = \cdot 66602$. We will also put $\varrho = 60$, $m = 5\frac{1}{2}$, and $m' = 2\frac{3}{4}$, and $t = 100$ years $= 3155760000$ seconds. These values will give $nt - u = 50 \cdot 17$ miles for the amount the Earth would be retarded at the Equator in a century, upon the hypothesis that the whole nucleus is covered with water. But as the waters of the ocean do not completely assume the spheroidal form on account of the continents, which occupy about one-fourth of the surface, we must deduct one-fourth from the value of $nt - u$, and we shall then have for the effect of the Moon in one century

$$nt - u = 37 \cdot 44 \text{ miles} \qquad (18)$$

Now if in the equation (17), L be increased or diminished, h will be increased or diminished in the same ratio; hence the effect of any other body in changing the value of $nt - u$ will be as the square of its effect in causing tides. But this effect of the Sun is $\frac{4}{7}$ths of that of the Moon, and consequently its effect upon the value of $nt - u$ will be $\frac{9}{49}$ of that of the Moon. We shall therefore have for the effect of both Sun and Moon,

$$nt - u = 44 \cdot 45 \text{ miles} \qquad (19)$$

If the motion of the Earth were retarded by this amount it would cause an apparent acceleration of the heavenly bodies in their orbits, which in the case of the Moon would amount to 84″ in a century. And as no such acceleration has been observed above what is accounted for otherwise, the rotatory motion of the Earth must be nearly uniform, and the above

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effect of the Sun and Moon must be counteracted by the gradual contraction of the Earth through a loss of temperature."

And, in conclusion, the author calculates that, to counteract the foregoing acceleration of 84″ per century, there is required a contraction of about 1·017 feet in the Earth's radius during a century, upon the hypothesis of an equal contraction throughout the mass.

Leyton Astronomical Observations.

In revising the proof-sheets of the above volume, I find an error in the arrangement of Comet Observations has escaped my notice; at p. 76, the observations for 1864, Feb. 8, 9, 16, 18, and March 1, belong to Comet V. and not to Comet IV.

This is immediately evident by turning to the comparison-stars at p. 95.

C. G. TALMAGE.

Mr. Barclay's Observatory, Leyton.

ERRATUM IN THE LAST NUMBER.

In the list of Fellows elected, *for* George Hurst, Esq., *read* George Hunt, Esq.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *June* 8, 1866. No. 8.

Rev. CHARLES PRITCHARD, President, in the Chair.

Ebenezer Little, Esq., 18 George Street, Tower Hill;
W. H. Bayley, Esq., 25 Cambridge Square, Hyde Park;
Richard A. Proctor, Esq., 3 Collingwood Villas, Stoke, Devon;
Henry Allason Fletcher, Esq., Millgrove, Whitehaven; and
Thomas A. Hirst, Esq., Ph.D. University College, London,

were balloted for and duly elected Fellows of the Society.

On the Effect produced by the Angles of Position of Double Stars on the Results of Micrometrical Measures of them; with a Description of a Method by which such effect may be avoided or removed. By the Rev. W. R. Dawes.

Very soon after I commenced my measurements of double stars in the year 1830, I became aware of a tendency to obtain a different result in position when the line joining the centres of the stars was nearly *parallel* to the line joining the centres of the eyes, from that which was obtained when those lines were nearly *perpendicular* to each other; and a still more decided difference was found to prevail when those lines formed a very *oblique* angle. In discussing the points connected with these differences it will be convenient to call the position when *parallel* to the line joining the centres of the eyes *horizontal*; when *perpendicular* to that line, *vertical*, and when nearly half way between the two, *oblique*. Thus, a double star whose angle of position is near 90°, or 270°, would, when on or near the meridian, have what may be called a *horizontal* position;

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if the position were about 0° or 180° it may be termed a *vertical* position; and if between 30° and 60° from the horizontal or vertical, the position would be *oblique*.

A tendency to a very considerable difference between horizontal and vertical measurements may exist without the observer being at all aware of it; and such a difference may to a great extent vitiate a large mass of observations without being suspected; except perhaps by comparison with the results obtained by other observers. But in such cases there will always be the great difficulty of determining which are the measures that have the strongest claim to be considered as true and standard ones.

In my own case I soon became aware of some difference in the results, which seemed to arise from the relative position of the stars; and the way in which I discovered it was this:—An *oblique* position seemed to me quite unsuitable for placing the wires of the micrometer parallel to the imaginary line joining the centres of the stars; and certainly, if it were wished, by aid of a ruler, to draw a line parallel to two points on paper, no one would, I should imagine, prefer to place the points in an oblique position for that purpose; but would, if possible, place them either in a vertical or horizontal position. When therefore I wished to measure such a star as *ε Bootis*, whose position-angle was about 50° from a horizontal and 40° from a vertical position, I performed the measurements when the star was sufficiently to the *east* of the meridian to bring the components nearly into a vertical line, and then obtained as nearly as might be an equal weight of observations when the star had arrived at a western azimuth sufficient to place its components nearly in a horizontal position; the large northern declination of the object permitting the two sets to be made within a few hours. Between the two results thus obtained I usually found a difference in the same direction, and pointing decidedly to the different positions of the star as its cause. I immediately set to work to devise means for ascertaining the amount of discrepancy arising from this cause; and also, if possible, for correcting the tendency to it. I considered that this would be preferable to proceeding with the measures, affected as many of them must be with an unknown amount of error; and then endeavouring by comparison of numerous results to calculate the amount of error in particular cases, and from this to devise a formula which might give a probable correction to all cases in which it was likely to be needed.

A very simple expedient soon occurred to me, which may be conveniently expressed in the form of directions to any one wishing to adopt it. Procure a moderately stout piece of cardboard. Out of this cut a triangular piece of any convenient size,—two of the sides being as accurately as possible at right angles to each other. The two acute angles may either be of 45° each, or more usefully of 30° and 60°. In this card make

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some small apertures; the larger ones with a very fine and sharp punch, and the smaller with a hot needle. It is well to have several pairs of holes, some of equal size and moderate distance to begin with, and others, for more advanced practice, of unequal size and smaller distance. Supposing the acute angles of the triangle to be 30° and 60°, it will be convenient to designate the side opposite the angle of 30° by B (the base), the other side by P (the perpendicular), and the hypothenuse by H. In this form of triangle there is no necessity for so many sets of holes to vary sufficiently the angles of position; for a great variation is produced in most of them by merely reversing the side of the card presented to the observer. The lines joining the centres of the apertures may be drawn at any angle with respect to B; but I should strongly recommend that one set of each kind (equal and unequal size, and at different distances) should always be made as exactly as possible parallel to B or P; and also one of each parallel to H. Of course, to prevent the confusion which might arise from too many sets of holes in one card, as many cards may be prepared as the observer may think convenient.

A card thus prepared and set up on the frame of an ordinary window may be viewed with a small telescope in a room of moderate size. I employed an excellent 2-foot object-glass by Dollond, aperture 1·6 inch, mounted in a pretty stout tube, with another sliding into it having a screw which fitted my parallel-wire micrometer, also by Dollond. This was firmly fixed in a stout clip upon a floor tripod; the clamp of the clip holding the tube so tightly as to prevent any accidental rotation on its axis.

The use of the apparatus is now so obvious as to need little further explanation. I first began with an equal and moderately wide pair in a vertical position, taking a few measures of it with the card standing on the side B; and then an equal number of the same object placed horizontally by turning the card on to the side P. The means of several sets of careful measures in each position gave accordant results; the difference between the vertical and horizontal measures, even when not large, always lay in the same direction; and thus I was quickly led to perceive its cause and its amount on objects variously constituted. Having thus determined the existence and the amount of the errors, I endeavoured to remove the cause of them by taking measures as accurately as possible in a vertical position, and then altering the reading of the micrometer 90° from the mean, the card being also turned from B to P. This at once revealed the error of judgment, which I have always considered to belong to the horizontal and all other positions rather than to the vertical, which were, at all events pretty uniformly the most accordant among themselves.

Each observer may, of course, pursue some such plan of observation on any number of *oblique* positions; and thus en-

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deavour to divest his judgment of all discrepancies attaching to them. But it presently occurred to me that such a process would be unnecessarily long and laborious; and that at the same time an equally satisfactory result might be obtained in a simpler way, namely, by the use of a small prism attached to the eye-piece between it and the eye. This I accordingly had done by Dollond; and found it work so satisfactorily that I strongly recommended its use in the Introduction to my first series of Double-star Measures, which was read before the R.A.S. in 1834, and honoured with a place in vol. viii. of the *Memoirs*. This simple contrivance, so easily managed, enables the observer to place any double-star in any desired position with respect to a vertical or horizontal line; and in my own observations I have frequently resorted to it even with stars whose position is not very oblique (as *e.g. Castor* and *γ Leonis*), in order to assure myself that I still retain the uniformity of judgment which I acquired by practising with the artificial stars in the cardboard. I have however confined myself entirely to the vertical and horizontal positions.

It is a remarkable circumstance that that excellent and experienced observer, M. Otto Struve, after having accumulated an immense mass of double-star measures, became aware that they were liable to a considerable error, in many instances at least, from obliquity of position; and in order to ascertain its amount and the law by which it was governed, he instituted a careful series of measures of artificial double stars set up at a considerable distance from the Poulkova Observatory, and observed with the large equatoreal. This is, of course, an incomparably more laborious plan, and can be pursued only under extremely favourable circumstances. There must also exist other rare and peculiar combinations to render it at all possible to affix suitable objects at a sufficiently great distance and in a suitable light, &c. The results however, procured with great care and labour, proved very important, and showed the necessity in many instances of applying a considerable correction to the observed angle, and a formula is given for its computation. In his paper on this subject he refers to my recommendation of the use of a *prism;* but raises the objection that a prism might introduce errors of a different kind, and in every case might injure the quality of the telescopic images. "This," he adds, "is the reason why astronomers in general have not adopted the proposal of Mr. Dawes, which under certain circumstances is worthy of being pursued, because it might furnish interesting corrections (or modifications, *contrôles*) to the results found by another method."

With reference to this objection to the use of the prism, which would be formidable if well founded, I feel bound to say that, having during the thirty-five years which have elapsed since my first proposal and use of it employed prisms made by Dollond, Merz, Simms, and the late Andrew Ross, I have found

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that no one of them produced any perceptible deterioration of the image. I cannot therefore but conclude that this fear has no foundation sufficient to deter any observer from the use of this instrument; and nothing can be more easy of detection than a fault of this nature. Moreover, the light lost is so small that extremely delicate objects may be observed with it. M. Otto Struve further states that he is not aware that I have published any results affording a comparison between the results obtained with the prism and without it. It is quite true that I have not published any such; the reason of which was simply that I supposed the defect was probably restricted to my own observations, never having at that time (in 1830) heard of any observer who had detected a similar source of error. Neither was this point elucidated by any allusion to it in a correspondence which ensued between Sir John Herschel and myself. I therefore concluded that it was probably peculiar to my own observations; and having succeeded in correcting the tendency to it by the method explained in this paper, I did not suppose that the subject would possess much interest for other observers. Lately, however, my attention has been again attracted to the subject by a correspondence with one of our best double-star observers; and I have therefore thought it desirable no longer to delay the present explanation of my own successful method of overcoming the difficulty.

Hopefield Observatory, Haddenham, Bucks,
6 June, 1866.

Equatoreal Observations of R.A. of Mars and neighbouring Stars for the Determination of the Sun's Parallax made with the Equatoreal of the Observatory at Madras in the year 1862, and Meridional Observations of N.P.D. of Mars and neighbouring Stars made with the Meridian Circle. By N. R. Pogson, Esq., Government Astronomer. (Abstract.)

At the request of the Astronomer Royal, observations of the Planet *Mars* were made during the opposition of 1862, at rising and setting, with a view of illustrating the method suggested by him in the *Monthly Notices of the Royal Astronomical Society*, vol. xvii., p. 219, and again in vol. xviii., p. 277, for the correction of the Constant of Solar Parallax, rather than with any hope of being able to realise a trustworthy result, owing to the very imperfect instrumental means available for such observations. A similar proposal had been made to Major

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Tennant, Government Astronomer at Madras, in regard to the opposition in the year 1860, but had also been prudently declined by him on the very grounds now urged and foreseen by the present Astronomer before commencing the series under consideration.

The instability of the Madras Equatoreal; its damaged and shattered condition, owing to the effect of repeated storms, from the fury of which the worthless folding roof over it was literally no protection at all; its defective and in part useless illumination; all combined to render every possible exertion and precaution alike nugatory, and to reduce the attempt to a mere justification of Major Tennant's judgment on the former occasion.

Observations (114 in number) were, however, made, with 20 Comparison Stars, between September 22 and October 28, 1862; and though, for the reasons above stated, the result could not be expected to possess any actual value, it nevertheless proves clearly that the long-adopted value of the parallax requires to be increased by a considerable fraction of itself; and that the method, if effectually carried out by the aid of better instrumental means, is in itself good, and well worthy of the utmost care and preparation, when the time shall again arrive for repeating the measurements of *Mars* under more favourable circumstances than those existing at the Madras Observatory in the year 1862.

The final value of x (Correction to Parallax), viz. $+0''{\cdot}579$, is evidently too large; giving $\pi = 9''{\cdot}156$. The Comparisons of *Mars* with four of the twenty stars did not enter into the determination at all, in consequence of these being only taken on one side of the meridian. All other observations are, however, included, and possibly a judicious weeding out of all dubious comparisons might give a better result. It may also have been wrong to correct only the mean values of $\alpha' - \alpha$ for refraction and parallax; each individual comparison, or at least those of not more than ten minutes apart, being better taken separately. It is not, however, imagined that much improvement would have arisen from so vastly extended a system of corrections; and the failure of the attempt is probably attributable to the too low magnifying power employed (only 63) rather than to any other cause; and much as the unsatisfactory result is regretted, the author exerted his most earnest endeavours to do the best he could with the very imperfect and unsuitable instrumental means available. He remarks that any suggestions as to a better mode of treatment of the observations would be thankfully received and acted on at the earliest opportunity.

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Occultations of Stars by the Moon, and Phenomena of Jupiter's Satellites, observed at the Royal Observatory, Greenwich, from April 1864 to April 1866.

Occultations of Stars by the Moon.

Day of Observation.	Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
1864.				
June 26	62 Piscium, disapp.	Bright	13 22 29·6	R.
	(*a*) ϑ Piscium, disapp.	Bright	13 40 7·1	R.
	(*b*) 62 Piscium, reapp.	Dark	14 11 32·6	R.
	(*b*) ϑ Piscium, reapp.	Dark	14 40 27·5	R.
Dec. 5	κ Aquarii, disapp.	Dark	8 34 54·8	J.C.
	(*c*) κ Aquarii, reapp.	Bright	9 28 17·9	J.C.
1865.				
Mar. 3	ϑ^2 Tauri, disapp.	Dark	10 15 20·9	J.C.
July 3	α^2 Libræ, disapp.	Dark	9 58 18·0	J.C.
	(*d*) α^2 Libræ, reapp.	Bright	10 50 33·2	J.C.
8	(*e*) ζ^1 Sagittarii, disapp.	Bright	9 21 8·0	J.C.
Nov. 4	ϑ^2 Tauri, reapp.	Dark	10 36 44·9	J.C.
Dec. 30	115 Tauri, disapp.	Dark	7 34 58·9	L.
1866.				
Feb. 23	130 Tauri, reapp.	Bright	6 23 46·1	C.
27	h Leonis, reapp.	Bright	9 0 43·5	C.

(*a*) The star appeared to hang about $0^s{\cdot}7$ before disappearance. (*b*) Instantaneous. (*c*) A little separated from the limb when first seen. (*d*) The star appeared to hang on the limb about $\frac{1}{2}^s$ or $\frac{2}{3}^s$ after reappearance. (*e*) A very unsatisfactory observation.

Phenomena of Jupiter's Satellites.

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
1864.				
April 14	I	(*f*) Eclipse, disappearance	13 33 44·2	C.
May 7	I	(*g*) Eclipse, disappearance	13 44 21·3	J.C.
16	I	(*g*) Occult. disapp. bisection	9 58 35·4	J.C.
22	III	Eclipse, reappearance	9 34 39·0	E.
23	I	Occult. disapp. first contact	11 41 52·3	C.
	I	Occult. disapp. last contact	11 46 36·5	C.
	I	Eclipse, reappearance	14 9 7·2	C.
29	III	Eclipse, reappearance	13 33 19·1	C.
June 13	II	Transit, ingress, first cont.	9 2 8·0	C.
	II	,, bisection	9 4 37·6	C.
	II	,, last cont.	9 6 37·1	C.
	II	Transit, egress, bisection	11 20 40·3	C.
	II	,, last cont.	11 22 40·0	C.

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Day of Observation. 1865	Satellite.	Phenomenon.	Mean Solar Time. h m s	Observer.
May 22	II	(*h*) Occult. reapp. bisection	13 34 30·7	J.C.
June 11	I	(*h*) Eclipse, disappearance	13 12 17·8	J.C.
19	I	(*h*) Transit, ingress, bisection	12 6 54·8	J.C.
20	III	Eclipse, reapp. bisection	12 41 41·9	E.
28	I	(*h*) Transit, egress, last cont.	10 32 39·0	C.
July 25	II	Eclipse, reappearance	10 41 32·9	C.
29	I	Eclipse, reappearance	10 20 28·5	T.C.
Aug. 2	III	(*i*) Eclipse, disappearance	10 4 9·5	L.
Sept. 2	II	Occult. disapp. first cont.	7 51 50·5	E.
	II	,, bisection	7 53 20·3	E.
6	I	(*i*) Eclipse, reappearance	8 50 51·4	C.
7	III	Eclipse, reappearance	8 43 54·1	E.
14	III	(*k*) Occult. reapp. bisection	7 34 26·7	J.C.

(*f*) Unsatisfactory; cloudy. (*g*) Very uncertain; the image of the planet bad. (*h*) The observation very unsatisfactory; the planet very badly defined. (*i*) Somewhat uncertain, the satellite being very faint near *Jupiter*, which was low. (*k*) Unsatisfactory from haze near the horizon.

The initials D., E., C., L., J.C., R., and T.C., are those of Mr. Dunkin, Mr. Ellis, Mr. Criswick, Mr. Lynn, Mr. Carpenter, Mr. Roberts, and Mr. Chappell.

On the Change that would take place in the Elements of the Earth's Orbit by a sudden accession to the Sun's Mass.

By John James Waterston, Esq.

That the appearance seen in the Sun on the 1st of September, 1859, by Mr. Carrington and by Mr. Hodgson, indicated an accession to the Sun's mass is no doubt very generally admitted, but that the amount of that accession can have sensibly diminished the length of the year is an idea that may perhaps incur ridicule, not from its impossibility, but because the Greenwich observations must ere this have detected so remarkable a circumstance. There seems also to be an impression that the fall of such a planet as the Earth into the Sun "would, from the conversion of its previous mechanical energy, give out an awful blaze of light, such a blaze as . . . would scorch up in a moment all the inferior planets, and probably the Earth also, with everything upon it" (*Monthly Notices*, vol. xx. p. 89).

When this is put to test of figures, as at p. 197 of the same volume, such a catastrophe is clearly out of the question. Even if the blaze were persistent, the general rise of the temperature could not exceed 10° or 15°, but it could not be persistent, inasmuch as the extra power of radiation can only act for the short time taken to traverse the Sun's atmosphere before plunging under its surface, which observation shows to be not only in-

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candescent but highly mobile as a fluid; the blaze would then terminate, and the general temperature of that part of the Sun be very sensibly increased, yet not so as to increase the radiating power in a perceptible degree.

Take the extreme case of the whole potential radiating power of the Sun being thereby raised 1000°, even this is only $\frac{1}{12000}$th part of the potential temperature that sends heat to us sufficient to maintain a general average temperature over the surface of the Earth of about 500° above the absolute zero. Now this proportion of 500° is only $\frac{1}{24}$th of a degree, and this is the extreme maximum effect that can be reasonably expected from such a planet fall.

The phenomenon observed on the 1st September is thus quite consistent with what might justly be expected from an accession to the Sun's mass approaching in magnitude to our planet. Mr. Carrington describes the intensity of the blaze to be fully as great as if there had been a hole in the screen attached to the object-glass of the telescope; fully as great as the direct unscreened sunlight. Mr. Hodgson describes it as being most dazzling to the protected eye. Such descriptions warrant an estimate of intensity several hundred times that of the normal surface; enough if persistent to produce a considerable change of climate; but as persistence is physically impossible, nothing of the kind need be looked for. The only indication of the mass thus added to the controlling centre of the system which we can become cognizant of is the possible decrement in the length of the year.

The following is the process of computation I have employed to ascertain this. The resulting formula, it will be remarked, is extremely simple in consequence of the excentricity of the Earth's orbit being small.

If the mass equalled that of our globe the decrement in the length of the year is 130^s. This would cause a difference of $5''{\cdot}3$ in the longitude of the Sun at the end of the first year (September 1, 1860), of $10''{\cdot}6$ on September 1, 1861, and on September 1, 1866, the Sun would be $37''$ in advance of the position given in the *Nautical Almanac*. If the mass was less than this the difference would also be less in the same proportion.

In fig. 1 the major axis of orbit is P S A; the triangle formed by the Sun, Earth, and aphelion focus is S E F; the radius of curvature is R E, and the normal E N.

An addition of $\frac{1}{m}$th to the Sun's mass causes an immediate decrement of $\frac{1}{m}$th in the length of R E and a change in the length of the normal, but no change in the direction of R E or F E.

Suppose a line to be drawn from S, passing through N',

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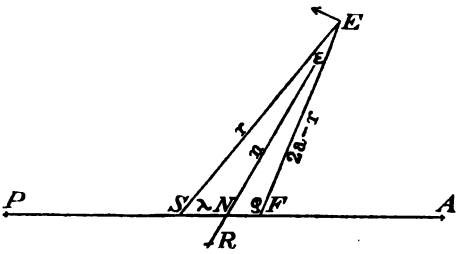

Fig. 1.

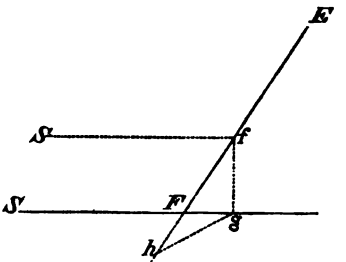

Fig. 2.

the new position of N, and produced to meet F E in f (fig. 2). Draw $fg \perp$ S F and make F h = F g.

The incremental change in the direction of the major axis is $\frac{fg}{SF} = \delta\lambda$. The decrement of major axis $2\delta a$ is $Ff = -2\delta p - 2\delta e$ (p being the perihelion distance P S and and $2e$ = S F). The increment of $2e$ is F g = F h, and the decrement of $2p$ is fh.

Let ϕ represent $\angle$ E F S, $\lambda = \angle$ E S F, and $\varepsilon = \angle$ E, $fg = 2e\delta\lambda$, $Fg = \cot\phi\, 2e\delta\lambda = 2\delta e$, $Ff = \operatorname{cosec}\phi\, 2e\delta\lambda = 2\delta a$.

With δa represented by x, we have

$$\delta\lambda = \frac{x\sin\phi}{e} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\delta e = x\cos\phi \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\delta p = x - x\cos\phi \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The radius of curvature ϱ is equal to the cube of the normal n $\left(= \frac{r\sin\lambda}{\sin(\lambda + \frac{\varepsilon}{2})}\right)$ divided by the square of the semiparameter s $\left(= \frac{b^2}{a}\right)$ hence

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$$\frac{\delta \varrho}{\varrho} = \frac{1}{m} = \delta\left(\frac{n^2}{s^2}\right) \cdot \frac{s^2}{n^2} = \frac{3\delta n}{n} - \frac{2\delta s}{s} \quad . \quad . \quad (4)$$

The first term

$$\frac{3\delta n}{n} = 3\delta\lambda \cdot \frac{\sin \frac{1}{2} s}{\sin \lambda \sin (\lambda + \frac{1}{2} s)} = \frac{3\delta\lambda}{\sin^2 \lambda \cot \frac{1}{2} s + \sin \lambda \cos \lambda} \quad (5)$$

In the triangle S E N, the angle s being small, we have

$$\frac{n \sin \frac{1}{2} s}{\sin \lambda} = e \text{ (\textit{nearly})} \text{ and } \sin \frac{1}{2} s = \frac{e}{r} \sin\left(\lambda + \frac{1}{2} s\right)$$

hence

$$\cot \frac{1}{2} s = \frac{r}{e \sin \lambda} - \cot \lambda$$

substituting the value of $\cot \frac{1}{2} s$ in (5) we have

$$\frac{3\delta n}{n} = \frac{3 e \delta\lambda}{r \sin \lambda} = \frac{3x}{2a - r} \quad . \quad . \quad . \quad . \quad (6)$$

The second term

$$-\frac{2\delta s}{s} = -\frac{4\delta b}{b} + \frac{2x}{a}$$

and since $b = \sqrt{a^2 - e^2}$ we have

$$-\frac{4\delta b}{b} = -4 \cdot \frac{a x - e \delta e}{a^2 - e^2} = -\frac{4x}{a} \text{ (\textit{nearly})}$$

hence

$$-\frac{2\delta s}{s} = -\frac{2x}{a} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and (5), (6), (7),

$$\frac{1}{m} = -x\left\{\frac{3}{2a - r} - \frac{2}{a}\right\} \text{ or } x = \frac{2a - r}{2r - a} \cdot \frac{a}{m} \quad . \quad . \quad (8)$$

Computing this for September 1, 1859, with $m = 354936$ we obtain $x = 261$ miles, which, compared with a and the length of the year, is found to represent a decrement of this length equal to 130 seconds.

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The change in the values of λ, e, and p, may be ascertained from (1), (2), (3), $\delta\lambda = -29''$, $\delta e = +137$, $\delta p = -398$.

Supposing many such planet-falls to have happened in the history of the Sun, and to take place at every different part of the orbit, the motion of the apses would be cumulative in the direction of the signs.

If the accession took place in July $x = -255$ and $\delta e = +255$.

If it took place in December $x = -281$ and $\delta e = -281$. The change of e is (+) at the upper half of the orbit and (−) at the lower.

Inverness, April 25, 1866.

Observations of the extraordinary Variable lately discovered near ι Coronæ. By E. J. Stone, M.A.

This star has been observed with the Greenwich Transit-Circle on every clear night since May 17. Its mean place for 1866, Jan. 1, is as follows:—

R.A.	N.P.D.
$15^h\ 53^m\ 53^s{\cdot}8$	$63°\ 41'\ 52''{\cdot}9$

In Argelander's *Bonner Sternverzeichniss*, Zone + 26°, No. 2765, will be found a star of 9·5 magnitude, whose mean place for 1855, Jan. 1, was

R.A.	N.P.D.
$15^h\ 53^m\ 26^s{\cdot}9$	$63°\ 39'\ 54''$

The mean place of this star for 1866, Jan. 1, without any allowance for proper motion, would be

R.A.	N.P.D.
$15^h\ 53^m\ 54^s{\cdot}5$	$63°\ 41'\ 49''$

There is certainly no star of the 9th magnitude near the variable. It is clear, therefore, that the variable is really the star No. 2765 of Argelander's Zone + 26°; and that at the time of observation in Argelander's sweep it was below the 9th magnitude.

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This star was seen, on May 12, by Mr. Birmingham, of Tuam, as a star of the 2nd magnitude.

It was observed by Dr. Schmidt, at Athens, on May 13. Later on the same evening it was observed also by M. Courbebaisse, at Rochefort. Mr. Barker saw it at London, Canada West, on May 14, and on May 15 it was independently discovered by Mr. Baxendell, of Manchester. Notice of the discovery was now circulated, and the attention of astronomers directed to the star. It is very remarkable that M. Courbebaisse is of opinion that the star could not have been conspicuous to the naked eye on May 11, and he is confident that such was not the case on May 9. Mr. Baxendell, also, is confident that the star was not sufficiently bright to attract attention with the naked eye on May 7. It is, however, extremely difficult to prove a negative, and we may even yet hope to receive some earlier observations of this star. The number of independent discoveries of this star is not a little remarkable, and proves how widely spread is the love of our science, and with what minute care and knowledge the heavens are nightly scanned.

The following table of estimated magnitudes contains all the observations which have come to my hands. It lays no claim to completeness, and is published merely with the hope of inducing those gentlemen who have made estimations of brightness to publish their results.

Estimations of Magnitude.

	Barker.	Baxendell.	Birmingham.	Carpenter.	Courbebaisse.	Daves.	Schmidt.	Stone.	Observers with Greenwich Transit-Circle.
May 12	...	...	2	...	...	...	...	...	...
13	...	...	...	...	2–3	...	3–2	...	...
14	3	...	...	...	...	...	...	...	...
15	...	3·6	...	...	...	...	3–4	...	...
16	...	4·2	...	...	...	...	...	...	...
17	...	4·9	...	4·5	...	...	...	...	...
18	...	5·3	...	...	...	4·7	...	4·8	...
19	...	5·7	...	...	...	5·2	...	5·0	...
20	...	6·2	...	...	...	...	...	...	...
21	...	...	...	...	...	6·3	...	...	...
22	...	...	...	7·2	...	...	...	7·2	7·5
23	...	...	...	...	...	7·2	...	7·8	7·5
24	...	...	...	...	...	...	...	8·4	8·5
25	...	...	...	...	...	...	...	...	...

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	Barker.	Baxendell.	Birmingham.	Carpenter.	Courbebaisse.	Dawes.	Schmidt.	Stone.	Observers with Greenwich Transit-Circle.
May 26	...	...	...	...	...	...	...	...	...
27	...	...	...	...	...	...	...	...	8·5
28	...	...	...	...	...	...	...	8·2	8·3
29	...	...	...	...	...	...	...	...	8·5
30	...	...	...	...	...	...	...	...	...
31	...	...	...	...	...	...	...	...	...
June 1	...	..	...	...	...	..	...	...	...
2	...	...	...	...	...	...	...	...	...
3	...	..	...	...	...	...	...	...	...
4	...	...	...	...	...	...	...	...	...
5	...	...	...	...	...	...	...	...	...
6	...	...	...	...	...	...	...	...	...
7	...	...	...	...	...	...	...	8·9	...

The Greenwich observations are based on Argelander's magnitudes.
On May 24 the sky was not in a good state for observing magnitudes.

It appears that the diminution of brightness was for some time at the rate of about half a magnitude a-day: towards the end of May the decrease was far less rapid. I have never noticed any traces of nebulosity in the star; it has always appeared to me to come up quite sharply to focus. This is also the opinion of all the observers at Greenwich.

On May 19 the Astronomer Royal's spectrum apparatus, by which the star-spectra are referred directly to the fixed lines of the Sun's spectrum, was mounted on the Great Equatoreal. On turning the instrument upon the star, it was at once seen that the ordinary star-spectrum was crossed by four bright lines; three of these lines were sufficiently bright for position measurement: the position of the fourth, as laid down in the diagram, was only estimated. The magnitude of the star at the time of observation was about the 5th.

The following excerpts from the observing-books are published with the sanction of the Astronomer Royal, who saw the spectrum himself on May 19:—

1866, May 19, 11^h.

Observers, E J. Stone and J. Carpenter.

Observations of Spectrum of New Variable in *Corona*.

The spectrum appeared to consist of two parts: one an ordinary star-spectrum, in which were traces of absorption-

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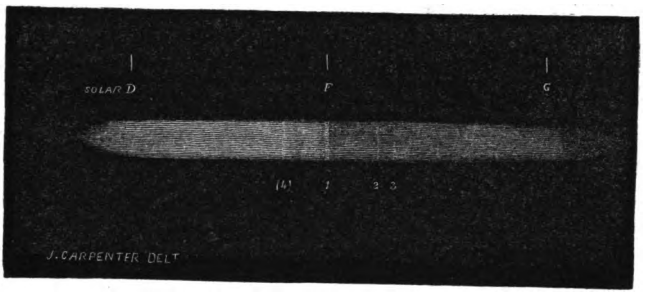
lines; the other, a discontinuous spectrum, consisting of four bright lines, of which the positions of only three were measured. The position of the fourth was fixed by estimation.



The visible spectrum extended from about D to half way between G and H of the solar spectrum. The contrast between the brightness of the lines and the adjacent parts of the continuous spectrum was very remarkable.

When the instrument is in adjustment, the reading for the solar line F is $89^r{\cdot}999$, and 20^r of the micrometer carries the index from F to G.

The following are the micrometer-readings for the three lines measured:—

For 1	$90^r{\cdot}486$	From 1 − 2 =	$5^r{\cdot}760$
2	$84^r{\cdot}726$	From 1 − 3 =	$7^r{\cdot}072$
3	$83^r{\cdot}414$		

May 20, 12^h.

Observers, E. J. Stone and J. Carpenter.

The following are the micrometer-readings:—

For line 1	$90^r{\cdot}407$	From 1 — 2	$5^r{\cdot}756$
2	$84^r{\cdot}651$		

The readings have each been diminished one revolution.

May 22, 11^h.

Observers, E. J. Stone and J. Carpenter.

Observation of absorption line in the spectrum of *α Coronæ* coincident with the solar line F, for verification of zero. The reading was

$90^r{\cdot}003$.

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Observation of line 1 of the spectrum of Variable: the reading was

$89^{r}\cdot658$;

observation difficult.

May 23, 11^{h}.

Observer, E. J. Stone.

The lines 1, 2, 3, and (4), were all seen, but the positions of only 1 and 2 were measured.

	r
Reading of Micrometer for 1 =	$90^{r}\cdot218$
2	$84\cdot325$
For 1 − 2	$5\cdot893$

May 24, 11^{h}.

Observer, E. J. Stone.

The reading of micrometer for line 1 was

$90^{r}\cdot346$.

Observation very difficult; strong moonlight, and at times haze.

May 28.

Observer, E. J. Stone.

The star was now of about the 8·2 magnitude. The moonlight was overpowering, but I have no doubt about the presence of the bright line 1. I tried several times to coax the micrometer up to the part of the field where I thought the line was. The micrometer was at each trial found reading about $90^{r}\pm$, or near the true place of the line.

June 7.

Observer, E. J. Stone.

The line near F was certainly still visible, but desperately faint.

The mean of all the readings for line 1 is

$90^{r}\cdot223$.

The reading for the solar line F is

$89^{r}\cdot999$.

The difference is too small a quantity to be answered for in observations of such difficulty. It is clear that the line 1

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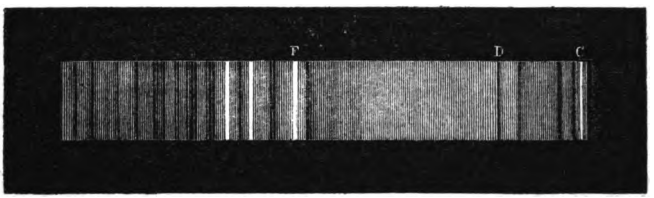
is either identical, or almost identical, in position with the solar line F. The diagram has been laid down assuming the bright line to be coincident with F. The unchanged character of the spectrum, when the star had decreased much below the 8th magnitude, appears to me a point of much importance.

It was the opinion of Mr. Carpenter and myself that from the 19th of May the brightness of the gaseous and ordinary spectra decreased in very nearly, if not quite, the same proportion.

1866, *June* 8.

Diagram of the Spectrum of Absorption and the Spectrum of Bright Lines forming the Compound Spectrum of the temporarily Bright Star near ε Coronæ Borealis.



This diagram, taken from the *Proceedings of the Royal Society*, is given, by the permission of that Society, in illustration of Mr. Huggins's letter to the Editor at page 275 of the last Number of the *Monthly Notices*.

Extract of a Letter from Mr. Graham.

The star in *Corona Borealis* which has attracted so much attention is a Variable star of wide range of magnitude, but is not new. It is marked in Argelander's comprehensive approximate Catalogue 9½ magnitude, and the place, referred to the mean equinox of 1855·0, is

R.A.	h m s 15 53 26·9	Decl.	26° 20′·1 N.
		N.P.D.	63 39·9

These for 1866·0 become

R.A.	h m s 15 53 54·3	Decl.	26° 18′·8 N.
		N.P.D.	63 41·9

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The place obtained by the meridian instruments at this Observatory, on the 17th instant., referred to the same epoch, is

R.A. $15^h\ 53^m\ 53^s\cdot85$ Decl. $26^\circ\ 18'\ 9''\cdot8$ N.
N.P.D. $63\ 41\ 50\cdot2$

Remarkably enough, in Wollaston's Catalogue, the epoch of which is 1790, there is an object recorded thus:—

R.A. $14^h\ 51^m \pm$ N.P.D. $63^\circ\ 29' \pm$

with the following note:—

"Double (Hers. v. 75) v. v. uneq. . . dist. 41″ 12‴ . . . pos. 16° *s.f.* It is really quadruple, for the small star is double, and there is a still smaller at about 30° *s. p.* the small ones."

This place reduced to 1866·0 becomes

R.A. $15^h\ 54^m$ N.P.D. $63^\circ\ 42'$

which also accords with the star in question.

Another circumstance worthy of record is that a nebula is marked on Cary's large celestial globe as nearly as possible in the very spot occupied by this star, and which is not in Herschel's Catalogue.

Cambridge Observatory,
25th May, 1866.

The New Variable Star near ε Coronæ.

By G. F. Chambers, F.R.A.S.

I should hardly have ventured upon troubling the Society with this communication had it not been for some remarks on the new star, made by Messrs. Hind and Talmage in letters addressed to a daily London newspaper; it seems to me that in dealing with scientific matters every observation *bonâ fide* made, however seemingly discordant with previous ones, should be placed on record. The following are my notes:—

May 21. Turned telescope (aperture 4 inches, powers 40 and 60) on the assigned place of the new star, and found the same immediately. Made a diagram of the neighbourhood. (x) is the stranger. It is about equal in brightness to (y), a star south of and in the field of ε *Coronæ*, but is brighter than (z), star south preceding π *Serpentis*. Hind speaks of the new star as free from colour, but to my eye it appeared from the very first moment of my seeing it to have a distinct pale orange tinge. It also struck me as particular sharp and well defined.

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Talmage however says the contrary; that, on the 18th at any rate, it was surrounded by a hazy nebulosity 30″ in extent. My own idea would be that if ever I had seen a star less likely under superior aperture to reveal a trace of nebula, this is that star. I hope to find light thrown on these discrepancies in some of the many papers which I suppose will be read at the next Meeting of the Society.

May 22. New star less bright and orange tinge less strong, but I consider perceptible, though the Rev. T. W. Webb, who was with me, did not notice it. He quite concurred however about the sharpness of the star's image.

June 2. The star has considerably diminished in size, and the orange tinge is no longer striking; I do not however consider the star destitute of a yellowish cast.

The two stars *ε Coronæ* and *π Serpentis* are I see rated in the Catalogues each as of $4\frac{1}{2}$ mag., but it occurs to me as just worth mentioning that the latter is considerably less bright than the former.

Attention has, I believe, long ago been drawn to the fact that no inconsiderable number of the important known variables present at their maximum red, orange, or yellow lines. It seems to me that this circumstance is worthy of more notice than has hitherto been given to it, and I cannot but fancy that some interesting but recondite physical law lies concealed here under the surface.

The Observatory, Sydenham, Kent,
June 4, 1866.

On a Supposed Observation of the New Variable near ε Coronæ.

(*Extracts from a Letter of Sir J. F. W. Herschel, Bart., to one of the Secretaries.*)

"At the suggestion of the Astronomer Royal, I send you herewith, for communication to the Royal Astronomical Society, a sheet pricked off and accurately copied from the original, containing my naked-eye estimations of the magnitudes of all the stars which I was able to discover on the night of the 9th of June, 1842, within the areas of five triangles, marked out by the stars *η, ζ, β Herculis, γ, β Serpentis, α Coronæ*, and *β, δ Boötis*.

"Within one of them occurs a star marked as of magnitude 6′, that is to say, according to the system of notation adopted, $6\frac{1}{3}$ or 6·7 magnitude, as distinguished from 7·6 (a magnitude clearly distinguishable by the naked eye in the absence of the Moon) and whose place, as laid down by allineations with the neighbouring stars *δ, ε Coronæ* and *π Serpentis*, agrees so nearly with that assigned to the star whose recent outbreak has

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excited so much attention, that I cannot help believing it to be the same."

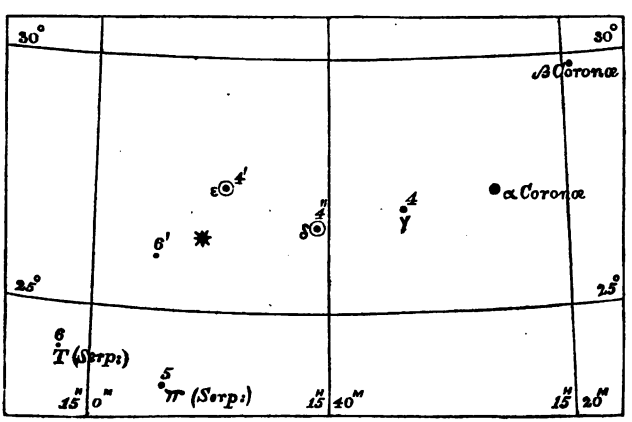
"The sheet I annex is copied from one out of a great series of similar ones, forming part of a series of naked-eye observations for the estimation of magnitudes and detection of variable stars, carried out in both hemispheres; in which the heavens were divided out into 738 triangles, with a design of mapping into them, *seriatim*, every visible star. The working-sheets were, in each case, pricked down from Bode's Atlas, and all the leading stars (including all of the 5th and a great many of the 6th magnitudes) laid down in that Atlas pricked in."

"The meridians and declination circle, drawn on a sheet, correspond, of course, to those in Bode's charts and to the epoch of January 1, 1801, adopted in that work. The stars not in Bode's charts (which are very numerous) were all corrected with as much care and endeavour at precision as could conveniently be bestowed on them in a work of the kind, but of course their positions are open to a good deal of error. In the case of the star in question however there can be no mistake as to the star, there being no other of equal brightness within a degree and a half of its place. If it be not the new star it is a Variable star which merits attention for its own sake.

"In an old celestial globe by Bardin, in my possession, containing 'the positions of 6000 stars, clusters, nebulæ, &c., &c., laid down from the latest observations of Maskelyne, Dr. Herschel, the Rev. F. Wollaston, &c., computed and reduced to the year 1800,' I find a star marked as 9th magnitude, laid down exactly in the place of mine of 6th mag."

Collingwood, May 28, 1866.

Note.—A portion only of the sheet has been engraved. The position of the variable is inserted and marked thus ✱.



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On the Advantages gained by substituting a Reflecting Prism for the Diagonal Mirror in a Silvered-Glass Speculum.

By John Browning, F.R.A.S., F.M.S.

In the paper which I read at the Meeting of January 9th, on a new method of mounting silvered glass specula in reflecting telescopes, I recommended the use of a prism, in preference to a plane silvered glass diagonal mirror, for reflecting the image into the eye-piece. Since then many objections have been made to the employment of a prism for this purpose.

It has been said, firstly, that dew is deposited on a glass prism much more readily than on a metallic surface. Secondly, that, in consequence of three surfaces, instead of only one, being brought into action, and the image having to pass through the glass, thus being influenced by all its defects, the difficulty of making the prism perform as well as the plane mirror is almost insurmountable. Thirdly, that the prism reflects no more light than the silvered mirror. This last statement has been made on Steinheil's authority, whether justly or not I cannot say.

To these objections the reply may be made, that if dew be deposited on the two front surfaces of the prism, it can readily be removed while the back or reflecting surface may be hermetically sealed, and so effectually protected from any deposition taking place.

Should moisture appear, however, on the surface of a silvered glass reflector, as is well known, it will greatly injure, if it does not entirely destroy, the film.

It is evident that any deterioration of the surface of this mirror will be much more prejudicial than the same amount of injury to the surface of the large speculum, from the cone of rays being concentrated on the small surface; and its exposed position near the mouth of the tube renders it far more liable to be injured in this manner than the large speculum. I have not made any experiments to determine the exact amount of light reflected relatively by the prism and the silvered glass mirror, because the amount would vary with every silvered mirror tested; but I think, simply viewing a white or greyish cloud reflected by the two placed side by side, will convince any person that the prism has a decided advantage.

But, besides this inferiority in reflecting power, the silvered diagonal mirror possesses another and far more serious defect; it does not reflect white light unchanged. I have experimented upon this property of the silvered mirrors with the spectroscope, and think that I can perceive indications of a deficiency in the green rays of the spectrum. The simple experiment represented in the following diagram, however, shows the change produced by such a mirror more plainly than any other I have devised.

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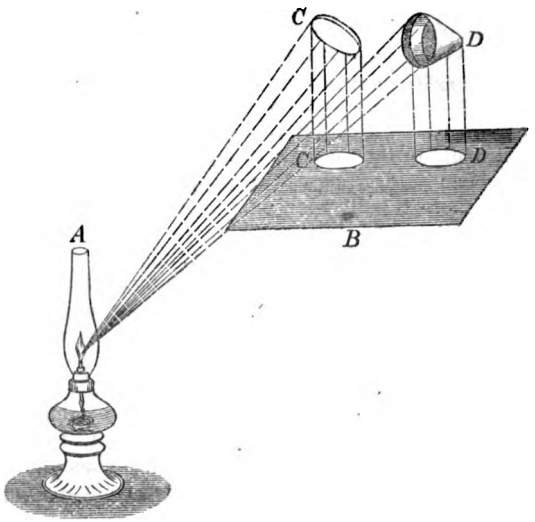
A is a paraffin lamp; B, a sheet of white card; C, a plane oval mirror silvered on the front or lower surface by Liebig's process; D, a prism made of very pure white optical crown-



glass. The sides of this prism, which are presented to the lamp and card respectively, are ground circular.

The prism and mirror are held, or supported on stands, about three feet from the lamp, and the height of the lamp flame from the card.

Under these circumstances two circles of light may be produced on the card,— one formed by the mirror, the other by the prism.

On comparing the two, it will be found that the circle produced by the prism is white, while that produced by the silvered mirror is strongly tinted with a reddish chocolate colour. This colour is greatly increased if the experiment be varied by causing the reflected light to fall on two silvered mirrors in succession before it forms the circle on the paper.

These are the conditions which obtain in the reflecting telescope, the large speculum representing the first, and the small diagonal mirror the second, reflexion.

The effect of these two reflexions in the reflecting telescope, as usually constructed, is to communicate a reddish tinge to the stars and planets.

Some difficulty has been experienced in attempting to use a telescope mounted in the usual way, for taking photographs; probably the substitution of a prism for the small diagonal

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mirror would lessen the difficulty which has been experienced in such experiments.

I have recently had the pleasure of making a very large reflecting prism, $2\frac{1}{2}$ inches in the minor axis, for our late excellent President, Mr. Warren De La Rue; it is now before you. This prism I have submitted to the severest possible tests, and I hope its performance will prove that such prisms are worthy of general adoption.

Note.—Jamin has published an account of some experiments "on the colour communicated to light by successive reflexions from the surfaces of various metals."

RECENT PUBLICATIONS.

Let og Noiagtig Methode &c. Easy and accurate Method for the Determination of the Latitude and Longitude at Mid-day, together with the Error of the Compass, without the help of Logarithms. By J. J. Astrand, Director of the Observatory of Bergen. With three Numerical and two Graphical Tables. Bergen, 1864.

The determinations are effected by means of two circum-meridian altitudes and the meridian altitude of the Sun: thus, if h_1, h_2, are the altitudes before and after the passage of the meridian, and θ_1, θ_2, are the corresponding hour-angles east and west of the meridian (reckoned in time); and if h_0 is the meridian altitude, then the differences $h_0 - h_1$ and $h_0 - h_2$ are proportional to θ_1^2 and θ_2^2, and, consequently, the hour-angle $\frac{1}{2}(\theta_2 - \theta_1)$ at the mean between the two times of observation is given by the formula

$$\tfrac{1}{2}(\theta_2 - \theta_1) = \tfrac{1}{2} \cdot \frac{\sqrt{h_0 - h_2} - \sqrt{h_0 - h_1}}{\sqrt{h_0 - h_2} + \sqrt{h_0 - h_1}} (\theta_2 + \theta_1)$$

where $\theta_2 + \theta_1$ is the interval between the two observations. But the quantity $\frac{1}{2}(\theta_2 - \theta_1)$ is obtained, not by actual calculation, but by means of one of the graphical tables, which, for given values of the differences, $h_0 - h_1$ and $h_0 - h_2$, gives the corresponding value of the coefficient of $\theta_2 + \theta_1$ in the foregoing formula. A correction for change of declination is introduced by one of the numerical tables, and the true local time at the mean of the two times of observation is thus obtained. Thus, in one of the examples given by the author we have

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Interval of the observations $44^m\ 36^s$	=	2676^s
$h_0 - h_1$	=	$39'\ 25''$
$h_0 - h_2$	=	47 0
Graphic Table I. gives multiplier	=	0,022
$2676^s \times 0{\cdot}022$	=	$58^s{\cdot}9$
Table III. gives correction	=	+ 5·6
True time	=	$1^m\ 4^s{\cdot}5$

which, corrected for the equation of time and compared with the Greenwich time, as shown by the chronometer, gives the longitude. The foregoing is a sufficient explanation of the principle of the method.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *Supplemental Notice.* No. 9.

Extract of Letter from Professor F. Kaiser, of Leiden, to the Astronomer Royal, dated 1 August, 1866.

From my investigations it appears that your double-image micrometer has far too little attracted the attention of Astronomers.

The many discrepancies one discovers in the measurements of double-stars that have been performed by different Astronomers have induced me, a year ago, to measure the principal double-stars, as well with your double-image micrometer as with the wire-micrometer. I wished to measure each star with each micrometer, on at least five different days; but the air was never so tranquil as was necessary for my measurements, and if I would make any progress, I was obliged to measure even when the dispositions of the air made measuring almost impossible. I have already closed my measurements on several double-stars with both micrometers, and, as a proof, I give you in the adjoining table the results for all the double-stars I measured, of which the distance is less than 12″, with exception only of two, the measuring of which has taken place under too bad circumstances. I have not yet had time to reduce accurately the measurements performed with your micrometer, and, in the adjoining table, I have provisionally adopted for the value of a revolution of the screw the constant number 7″·50. A more accurate reduction cannot modify the results above a few hundredth parts of a second. The harmony between my first results is greater than generally exists in micrometer measurements on double-stars, and it would have certainly been still much greater if the air had not thwarted me. In Mr. Main's measurements with the Oxford heliometer, published in the *Radcliffe Observations*, and in those with the heliometer of Königsberg of Dr. Auwers, published in the *Astron. Nachr.* No. 1393, I found sixteen that have taken place nearly at the same time. In eleven of those sixteen measurements the discrepancies in the distances are more than 0″·25. In *γ Androm.* the difference is 0″·69; in

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θ Serpentis, 0″·90; in *η Cassiop.* 0″·93; and in *β Cygni* over 1″·42. Luther at Konigsberg differed, when using the same heliometer, sometimes a full second from Peters and Auwers (*Astron. Nachr.* No. 1393). Very great differences are also found between the measurements performed with the wire-micrometer. The great harmony I obtained with two quite different instruments does prove that the discrepancies must far more be sought in the observers than in the instruments. The actual measurements on double-stars appears to me far too inaccurate for the consequences one will derive from them.

In the screw of the wire-micrometer, delivered by Mr. Merz, I found errors that amounted to 1″. A paper on that micrometer has been printed in the Periodical of the Royal Academy of Amsterdam.

	Star.	With the Wire-Microm.			With Mr. Airy's Microm.		
No. Σ	Name.	Epoch.	Dist.	Angle of Position.	Epoch.	Dist.	Angle of Position.
			°			°	
2737	ε Equulei	1865·85	1·02	103·0	1865·75	1·08	...
2583	π Aquilæ	5·84	1·34	117·6	5·79	1·44	...
2055	λ Ophiuchi	6·50	1·45	22·8	6·53	1·38	26·9
2262	τ Ophiuchi	5·68	1·51	247·8	5·57	1·50	250·9
948	12 Lyncis	6·28	1·64	316·5	6·35	1·59	317·3
1523	ξ Ursæ Majoris	6·45	2·08	87·8	6·39	2·10	86·7
2383	5 Lyræ	5·85	2·35	142·6	5·75	2·35	143·4
774	ζ Orionis	6·21	2·36	155·5	6·11	2·31	150·9
1877	ι Bootis	6·47	2·63	324·8	6·50	2·74	325·8
2382	ε Lyræ	5·85	2·95	20·6	5·75	3·12	16·3
1424	γ Leonis	6·24	3·09	110·7	6·33	3·15	109·9
2909	ζ Aquarii	5·85	3·11	158·9	5·75	3·34	157·4
1954	δ Serpentis	5·69	3·13	194·4	5·54	3·18	192·3
1009		6·28	3·30	337·0	6·34	3·23	335·1
1670	γ Virginis	6·46	4·01	165·9	6·38	4·23	164·6
738	λ Orionis	6·21	4·09	41·3	6·06	4·33	42·1
2140	α Herculis	5·69	4·64	115·4	5·72	4·56	115·7
1223	φ^2 Cancri	6·24	4·71	214·1	6·33	4·86	215·3
1110	α Geminorum	6·27	5·16	240·8	6·37	5·23	241·6
1888	ξ Bootis	6·46	5·23	298·6	6·42	5·19	300·8
2272	*p* Ophiuchi	5·68	5·31	100·4	5·56	5·34	100·9
1196	ζ Cancri	6·24	5·33	318·3	6·33	5·51	318·7
1224	IV Cancri	6·24	5·65	220·0	6·33	5·85	221·3
1864	π Bootis	6·45	5·73	100·6	6·40	5·68	101·
1965	ζ Coronæ	5·69	6·01	303·1	5·72	6·00	302·7
1998	ξ Libræ	6·49	6·93	70·5	6·53	7·05	70·0
2760		5 87	9·67	45·3	5·96	9·60	45·5
2737	ε Equulei	5·85	10·45	76·4	5·75	10·26	75·6
752	ι Orionis	6·21	10·87	140·6	6·09	11·13	140·5

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On the Depression of the Barometric Column by the Vapour of Mercury. By Gen. R. Shortrede.

It is well known that when a barometer has been stationary for some days, globules of mercury are to be seen adhering to the sides of the vacuum part of the tube, and more abundantly the longer the interval. These globules are formed by the condensation of the mercurial vapour which is given off in greater or less quantity according to temperature, and condense on the surface of the glass when it becomes colder, in the same way as moisture is deposited on glass.

The quantity which rises is doubtless very small, but it is not altogether inconsiderable; and though usually disregarded, I believe its elasticity is such as very sensibly to depress the height of the mercurial column. In India, where I had a good deal of experience with barometers, I found I could not get a true reading till I had condensed the vapour by tilting the barometer so as to make the mercury click against the top of the tube. The difference of the readings before and after this operation was generally from 10 to 20 thousandths of an inch, and on one occasion I found it as much as ·023. My barometers were in unexceptionably good order. They had been repeatedly boiled, one of them more than twenty times, and so perfect was the vacuum that on being put up after lying for some hours in a horizontal position, the mercury, by electrical attraction, would adhere to the top of the tube, and not separate till shaken by tapping. The tubes of 32 inches remained full at Pana, where the usual height is 28, and on the top of Singi, where the proper height was 26, the tube remained full.

Even in this country the vapour causes a sensible depression. In the rooms of the Meteorological Society at Edinburgh some years ago, I said that, in my opinion, none of the barometers were showing the true atmospheric pressure. On taking readings of two or three of the instruments which admitted of being tilted there was a difference of from 4 to 6 thousandths before and after. In the cold weather of winter this depression may probably be not more than 1 or 2-thousandths, but in summer I have no doubt that it will frequently amount to 8 or 10 thousandths.

At Greenwich, on Saturday, I noticed and showed to others that the vacuum part of the barometer for outside indications was studded with minute globules, as was also the cistern, the sides of which under cover were plentifully studded, and on the iron float was one large globule, occasioned probably by the vapour, from its higher conducting power, condensing more readily upon it than on the glass.

The subject is one that seems to deserve investigation, and if my idea is correct a correction will be required on the registrations photographed at Greenwich.

6 *June*, 1866.

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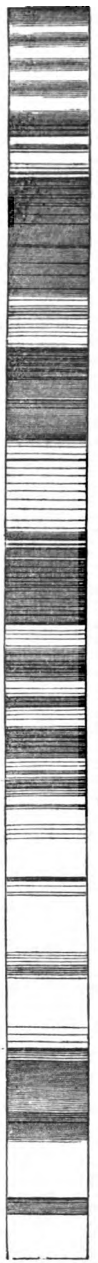

Spectrum of *Antares*, Rome, July 1866.
D Sodium line. *b* Magnesium line.
Beyond this, to the right, the spectrum, with the large prism, is very faint. It shows some bands with the new spectrometer.

On the Spectrum of Antares.
By the Rev. Father Secchi.

I have the honour of presenting the Royal Astronomical Society with a drawing of the spectrum of *Antares*. This has been made by actual measurement, but on a different scale of that of *α Orionis*, since the prism is different; this prism is one of Amici, with direct vision, but larger, and made by Secretan, of Paris. The points of reference are the lines D of the sodium and *b* of the magnesium. On the figure the lines have been so traced that a numerical scale can be obtained immediately by simply applying a metrical rule on the paper; one centimeter corresponding to one revolution of the screw of the micrometer.

Lately I have found that, for observing the most feeble spectra, it is sufficient to place between the object-glass of the telescope and the eye-piece a cylindrical lens of two or three inches focus, and behind it (between the eye-piece and the cylindrical lens) a prism of Amici of strong dispersive power, as those made by Hofman for his pocket spectroscopes. The image of the star becomes a line beautifully decomposed into its elementary colours, and the image can be seen with the common eye-pieces, and the distances of the lines measured with the common filar micrometer. With a power of 200 I have decomposed the bright bands of *Antares* into their luminary elements, as with the large spectroscope. I have also had beautiful spectra, and capable of giving the black lines of the green in some stars of the 8th and 7th magnitude. Applying this prism to a telescope of $2\frac{1}{2}$ inches aperture, I have been able to see the black lines of *α Aquilæ* and the bands of *Antares*. This system may be useful for amateurs, when no absolute but only relative measures of the lines are wanted.

Rome, August 1, 1866.

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On the Craters of the Moon. By Richard Hodgson, Esq.

Most observers of the Moon have occasionally seen the hollow craters change their appearance and become solid high mountains, while the ranges of mountains become hollow crevasses. Generally it has been attributed to a fatigued vision, and probably by removing the eye from the telescope, in the course of a few minutes the illusion passes away. Many times have I been teased with these appearances, and it occurred to me that the delusion arose from the position of the shadows being the reverse of what we usually see, and that in the fatigued eye for the time the retina refuses to fulfil its duties and invert the image, for the instants the craters are seen as mountains; fortunately this can be easily demonstrated, by applying a reflecting diagonal solar eye-piece, with a power of 200, to the edge of the Moon when the craters are prominent, and observing first on the one side of the telescope in which the craters appear hollow, and then revolving the whole eye-piece 180 degrees, and observing from the other side of the telescope, the craters are at once transformed to mountains and the ranges of mountains to crevasses, and no power of will can change them back to their proper forms, though the revolution of the eye-piece to its original position at once restores them.

Chingford, Essex,
5th June, 1866.

Occultation of Jupiter's Third Satellite. By Capt. Noble.

I last night observed the reappearance of *Jupiter's* third satellite from Eclipse at $8^h\ 41^m\ 30^s{\cdot}1$ L.M.T. It took place quite sharply and suddenly; and I had but a short time before obtained my clock-error and rate very accurately by a transit of *γ Draconis*, my transit instrument being very carefully levelled mechanically, so as to eliminate all azimuthal error. Now the *Nautical Almanac* for this year states that this reappearance would take place at $8^h\ 43^m\ 55^s{\cdot}3$ G.M.T.; and what aggravates this remarkable discrepancy is, that my Observatory is actually some $17^s{\cdot}5$ East, or *fast* of Greenwich. I am desirous that this observation should be recorded in the Supplementary Number of the *Monthly Notices*, inasmuch as it will call the attention of other observers to this very curious anomaly, and will probably elicit records of observations of the same eclipse. I have written to Mr. Hind to ask whether it may not be a blunder of a computer? but shall look out very curiously for the exact time at which the reappearance

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was seen at Greenwich, assuming that an observation was made of it. I may just say that I employed my Ross Equatoreal of 61 inches focal length and 4·2-inches aperture, with a power of 154, in observing the phenomenon myself.

Forest Lodge, Mareefield, Uckfield,
August 18, 1866.

The New Variable near ι Coronæ. By J. Birmingham, Esq.

The following extract from a letter, dated Millbrook, Tuam, July 7, 1866, addressed to E. J. Stone, Esq., contains an account of the first observation of this remarkable star.

"On my way home from a friend's house, on the night of May 12, I was struck with the appearance of a new star in *Corona Borealis*. It seemed at least fully equal to α of that constellation in size, and was superior to it in brightness. Its colour appeared to me nearly white, with a bluish tinge; and, during the two hours that I continued to observe it, I detected no change in its light or in its magnitude. I did not perceive the yellow or orange seen by subsequent observers. It shone quite like the neighbouring stars, without any particular unsteadiness or flashings. I regret to say that my instrumental means of observation were limited to an ordinary telescope with a power of about 25. I could not be sure of the exact time of my first seeing it, as I was then on the road, some distance from home; but I am certain it was between 11·30 and 11·45 P.M. Tuam time."

There is contained in the *Comptes Rendus* of 30th July and 6th August, 1866, an interesting paper by M. Faye, on Variable Stars, with especial reference to the temporary star in *Corona*. The conclusions are stated as follows:—

"En resumé, les étoiles dites nouvelles ne méritent pas ce nom, leur apparition presque subite n'est qu'une exagération du phénomène ordinaire des étoiles périodiquement variables, lequel repond lui-même à de simples oscillations plus ou moins sensibles dans le phénomène de la production et de l'entrétien des photosphères de tous les étoiles. Ces phénomènes considérés comme successifs dans l'histoire d'une étoile prise à part, caractérisent les progrès de son refroidissement et le déclin de la phase que j'appellerai volontiers solaire ou photosphérique. Quand ils se produisent ainsi avec le caractère d'intermittences irrégulières de plus en plus séparées par de très-longs intervalles de temps, ils sont les précurseurs de l'extinction défini-

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tive, ou du moins de la formation d'une première croûte plus ou moins consistante. C'est pourquoi les phénomènes de ce genre ne se produisent que dans les astres d'un éclat déjà très-faible et n'aboutissent jamais à doter le ciel d'une belle étoile de plus."

Minor Planet ⑰ *Sylvia.* By N. R. Pogson, Government Astronomer at Madras.

A new minor planet, of about 11¼ magnitude, was discovered here on the morning of the 17th May, civil reckoning, by the aid of the same manuscript map which has already realised me the planets *Iris* and *Sappho*, and the four variable stars R, S, and T *Ophiuchi*, and U *Scorpii*. The new planet was, indeed, within the limits of my map of the last-named object in my long promised Atlas of Variable Stars. The first night's comparisons were made by means of the ring-micrometer of the Hartwell 5-foot telescope, so long and kindly lent me by the late Dr. Lee. All the rest were taken by the Boguslawski method, or difference micrometer; to my mind infinitely the best for all such extremely faint objects. Presuming upon its not having been previously found elsewhere, I have again selected a name from the list furnished me a few years back by Sir John Herschel, viz, *Sylvia*, the mother of Romulus.

The new Equatoreal by Messrs. Troughton and Simms will, I hope, be in use within a week hence, and with it the planet will be easily observable; but it is severe eye-straining for the Lerebours' Equatoreal, with the Moon above the horizon.

The Apparent Positions observed so far are as follows:—

1866.	Madras M.T. h m s	Mag.	App. R.A. h m s	Log. Parallax Factor.	App. N.P.D. ° ′ ″	Log. Parallax Factor.	Comparisons.
May 16	14 47 51	11·7	16 15 15·69	+9·4967	107 28 48·9	−0·5973	10
16	15 28 12	..	16 15 14·54	+9·5972	107 28 49·2	−0·5664	10
17	16 10 41	11·6	16 14 27·70	+9·6810	107 28 59·0	−0·5467	16
18	14 22 56	12·0	16 13 45·05	+9·4054	107 29 8·1	−0·6325	18
19	13 37 27	11·8	16 13 0·39	+9·2143	107 29 20·8	−0·6466	18
20	13 33 27	12·0	16 12 14·07	+9·1984	107 29 31·4	−0·6475	20
22	13 28 22	11·9	16 10 40·50	+9·2419	107 29 55·1	−0·6455	20
23	14 12 46	11·8	16 9 51·90	+9·4565	107 30 8·4	−0·6257	20

The comparison stars employed have all been observed with the Meridian Circle of the Madras Observatory.

Madras, May 28, 1866.

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Minor Planet ⑱ *Thisbe.* By C. H. F. Peters, Esq.

(*Letter to the Astronomer Royal.*)

The following are observations of an asteroid discovered here on the 15th instant, near daybreak, but not made sure of before the 20th, on account of dark weather intervening:—

1866.	Ham. Coll. M.T. h m s	R.A. h m s	Decl. ° ′ ″	Log ($p\Delta$)″.	
June 15	15	20 26 4	−17 37 ..	..	..
20	13 56 31	24 39·65	25 54·8	9·9682	0·8886
21	12 49 50	24 16·67	24 7·5	0·4355	0·8802
22	13 12 13	23 50·76	22 20·8	0·3009	0·8843
23	12 57 14	23 23·87	20 41·0	0·3568	0·8829
24	14 26 26	20 22 52·94	−17 18 56·9	9·6898	0·8887

Interrupted by other engagements, I have not been able to accompany these observations with elements. The planet has the brightness of 11 mag., and being still far from opposition, it may be observed for a long while with meridian instruments. At the suggestion of a friend, the planet has received already the name *Thisbe*, as it is not likely that it has been seen before elsewhere.

Hamilton College Observatory, Clinton, N.Y.
June 25, 1866.

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